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EDITORIAL.

HOW TO WRITE A THESIS.

The value of a book should never be measured by its size, since this sort of reasoning is only too often at fault. Of course, as things go nowadays, we must have our money's worth or our commercial spirit is outraged; and when this happens woe betide the man who comes to us with the consolation that he, too, has been cheated. Now though the writer of these lines may or may not be as assertive as others in demanding full value for outlay of money in a book, he is quite sure he is not so insistent on bulk as are a goodly number of other readers, but pays some attention to contents; and is not too greatly depressed if the contents is crowded into a small space. A book that fits this argument to a nicety is Dr. H. D. Rolleston's "On Writing Theses for M. B. and M. D. Degrees," recently published by John Bale, Sons and Danielsson, London; for here we have very little bulk and a high quality that is well worth the price of one shilling—an insignificant sum, to be sure, at first sight—but not so insignificant when one takes into consideration that the pages are smaller in size than those in most books and their number but twenty-seven. One shilling for some twenty minutes' reading! Who would not cry out against the outrageous price.

But on closer study the kindly disposed reader, to whom we take it we are appealing, will find in these few pages much food for thought, if he happens to be one who has sought the reasons why medical papers are so deplorably lacking in those qualities which are, one might say, the life of all good literature. For on page 25 he will read, "Sir Michael Foster, whose textbook of physiology attracted a past generation almost as much by its manner as by its matter, had, his son tells me, made a critical study of Milton's prose writings. An eminent physician, whose writings are a

pleasure to read, had, I believe, saturated his mind with Addison's 'Spectator.' . . . Sir Clifford Allbutt offers many models, such as John Morley, George Trevelyan, Hardy, Barrie; and among medical writers, Sir Thomas Watson and Sir James Paget"; lines which should burn deep into his consciousness.

Now even though this imaginary reader will profit for the moment by this instruction, will he remember what preliminary work, in the matter of reading, he should do to achieve distinction in style when he sits him down to write an article for a medical publication? We doubt it, for the reason that directly he conceives an article of a medical nature, the perfection of its literary execution will be lost sight of, since his conceit, or his laziness, or his amour-propre, will stand him in good stead when he consoles himself with the thought that "really it's all of no importance since the article is just a medical one." This reasoning, we may add here, has done its work so effectively that, especially in our American journals, the things that are put forth in the name of science or medicine are, with few exceptions, a weariness to the soul of him who has been so misguided as to pay some attention to literary quality.

The matter of writing theses in this country is a negligible quantity; and perhaps our advancement beyond this means to an end—a doctor's degree—has much to do with our later efforts in the medical journals. Of course, as Dr. Rolleston points out, all theses in England and France are not written by the candidates; but even when "ghosts," men who eke out a living by writing and selling theses and scientific papers, are resorted to by the desperate students who are aware of their deficiencies, the coaching the candidate receives—he really only gets ideas which he has to dress up in his own individual way—is a lasting lesson. That this must be so is evident to everyone who has read the best English and French medical journals; for written large across the pages is a literary execution that is foreign to our publications. We may quarrel with the English on account of their insular heaviness, their mastery of detail, sometimes, it is true, carried to extremes, their interminable sentences; we may say of the French medical writers that too much artistry destroys the scientific nature of a paper, that what medical readers want is facts and not opinions that have the stamp of chauvinism; but, granting all this, must we not admit that by comparison with these publications ours are mediocre?

Someone has said that morality is a matter of geography, and we imagine that with this thought in mind the men who occupy the editorial chairs of our medical journals are quite disposed to say that the same thing applies to medical journalism. Have we not repeatedly heard of the superiority of our Eastern journals over their Western congeners?

Have we not willingly acquiesced in the matter, aware, as we must have been, that the medical culture in the East cannot be gainsaid? But was this humble spirit the outcome of a close study of the Eastern product? Or was it not rather the result of a mental superficiality that compelled us against our better judgment to grant certain journals the palm because of their geographical situation? Be the cause what it may, the facts, cold and hard, are declarative of the almost universal lack of scholarship in our medical journals and of the insouciance of even the "best" among them to what all European journals insist upon—namely, the thought of the writer so well clothed that grace of diction and a certain amount of classicism are paramount qualities.

THE THYROID GLAND.

Perhaps no field of medicine is more fraught with problems of practical and speculative interest than is that which deals with the elucidation of the mysterious activities of the so-called glands of internal secretion. Recent work points to the probability that all these glands, including thyroid, adrenal, hypophysis, pancreas and liver form a physiological combination, the proper functioning of which is absolutely necessary for the wellbeing of the body. A disturbance in any one of the parts of the intricate mechanism means a more or less severe derangement of some vital function of the whole. Diabetes mellitus, according to Von Noorden's theory, is always related to a pathological activity of the pancreas, either because of some injury to the organ itself or because the co-ordinating mechanism of the other glands of internal secretion is awry. Even though the absence of demonstrable anatomical lesions makes absolute proof of this contention impossible, the clinical manifestations and deductions are almost unanswerable. Practically in every disease associated with disturbed activity of any of the ductless glands there is an accompanying disturbance of carbohydrate metabolism, the nature of which depends on whether the original damage produces hyperactivity or hypoactivity of the gland.

In the elucidation of the theory of the interactivity of the internal secretions, the greatest amount of actual knowledge and the most intense stimulus to the general conception have arisen from a study of the thyroid gland in health and in disease. From an almost impossible chaos of ignorance and erroneous speculation, there has emerged in a few years a well-constructed working basis which, while no means completely satisfactory, yet offers the ward or laboratory worker a foundation on which to build a firmer structure. In the light of all that is known about

the pathological physiology of the thyroid it may seem strange to state that the normal physiology is far from being known. It is undoubted that the gland is necessary for proper health, and that its activity is in some way closely related to iodine metabolism, but beyond this we enter the labyrinth of theory. However, mere remembrance of cretinism, on the one hand, and of Basedow's disease, on the other, recalls striking pictures of conditions produced by hypoactivity or hyperactivity of the thyroid. That cretinism is caused by insufficient thyroid secretion is absolute, and that exophthalmic goitre is at least associated with too much of the same secretion is almost as absolute; and to-day medical science, accepting these two extremes as types, is attempting to clarify the mid pictures.

The object of this line of work being to make earlier diagnoses and more efficient treatment possible, it is not out of place briefly to review the results obtained in various places. Kocher, of Berne, whose opportunities have been unlimited, has been one of the most persistent advocates of the use of all means of study which will make the diagnosis of disturbance of the thyroid possible in the early stages, and his contributions have been classical. Properly to understand modern conceptions of the subject one must realize that in addition to the two extremes there exist many phases of disturbed activity, which may be due to too little or too much secretion or indeed to both. Starting with the assumption that in a well-developed case of exophthalmic goitre (or as the newer nomenclature would have it—hyperthyroidism) one finds goitre, tachycardia and other signs of trouble in the cardio-vascular system, nervousness, gastro-intestinal upsets, and eye signs, it is urged that the complete complex is by no means necessary for a diagnosis. The enlarged gland is always present, even though it may not be felt clinically; and this enlargement coupled with any one or two of the other symptoms should at least make the physician suspicious of the thyroid as the cause of the whole picture. In addition to the classical syndrome Kocher and others have added valuable diagnostic information in the form of careful blood-studies. It is maintained that in hyperthyroidism the differential blood count shows a distinct mononucleosis with a diminution of the polymorphonuclears. A further diagnostic point is offered from a study of the effect of iodine medication in cases of suspected hyperthyroidism: when the symptoms of such cases are actually caused by excessive thyroid secretion iodine medication invariably tends to increase the severity of the complaint. For this reason, it might be added, in passing, that Kocher has rigidly urged against such medical treatment except in the hyperthyroidism of puberty and other physiological states in which the en-

larged gland represents an attempt, on the part of the body, to compensate for the increased demands, and in which consequently iodine would assist Nature's efforts.

The relation of the thyroid to carbohydrate metabolism is still under study, but even now certain facts have been deduced which probably are of great practical value. It has long been known that some patients with Basedow's disease also had glycosuria, but the nature of the mechanism was not known. Now it has been shown that normally the thyroid has a stimulating action on the glycogenic function of the liver, and that in conditions of hyperactivity this stimulating effect may be overdone, thereby producing mellituria. However, it can be conceived that while the function may be overworked, it has not yet reached the stage of producing sugar in the urine, and this conception has been the basis for the so-called "sugar tolerance" test. A normal person can tolerate approximately 2 gm. of glucose per kilo body-weight, when given by mouth, without passing sugar in the urine; but the tolerance of a patient with hyperthyroidism would in all probability be more or less diminished. The test is extremely easy to perform, simply by weighing the patient and giving him in lemonade an amount of glucose equivalent to 2 gm. for every 2-1/5 lb. body-weight; then testing the urine voided three hours later for sugar. Of course, the condition of diminished tolerance for sugar exists in many diseases, and its presence is merely one of the many aids in forming an absolute diagnosis.

From the therapeutic standpoint, the importance of all studies leading to early diagnosis cannot be overestimated, for whether medical or surgical treatment be advocated success depends to a great extent on the amount of damage done the organism by the continued hypersecretion. Kocher and most authorities seem agreed that surgery offers the only rational cure to-day, but there are still those who urge *carefully controlled* medical attention in the way of complete rest, diet, and isolation. The success of the specific sera has not been as great in the hands of most physicians as it seems to be with the originators. It is not within the scope of this comment to criticize the various methods of treatment; we wish, however, to emphasize that no matter what method be followed, an early diagnosis is almost a necessity to success.

OPINION AND CRITICISM.

MENTAL CALISTHENICS.

Heinrich Heine, writing on the subject of the national traits of Englishmen, Frenchmen and Germans, says that the Englishman and the Frenchman are alike in this that they have a *present*, while the German has only a *past* and *future*. Now Mr. Arnold Bennett in his book, "Mental Efficiency,"* evidently coincides with the German poet; for according to his wise words, which are addressed to Englishmen, he dwells upon the present to the exclusion of all mention of the past and the future. And since Americans, by right of the language which they speak, belong to the great Anglo-Saxon family, we feel that it must be the present with us that is all-important, and that any divagation therefrom should be subjected to the corrective measures which Mr. Bennett would fain apply in the hope of its betterment.

To make the present worth while, Mr. Bennett advocates certain calisthenics for the brain, which, while not so severe as the gymnastics which enthusiasts apply to their bodies in the hope of emulating Lord Byron who wore "seven waistcoats, ran and played cricket till exhausted by excessive perspiration, and saw his ribs display Skin of no great Thickness and his clothes taken in nearly half a yard," are productive of much good. As can be readily gathered after reading the opening chapter, the mind is a very much neglected part of the human economy; for when inefficiency occurs,—that is when this important member of the body creaks and thereby shows it is out of joint,—scant attention is paid it. But it is an altogether different matter when the body gives evidence of too much lassitude, or too great an accumulation of fat, or the joints by their stiffness make plain that age must be fought off by Sandow-like strenuousness; then there is a mad rush to some "physical culturist" to learn the art of prolonging youth until the age of seventy. As the author pointedly puts it: "Will he remark with genuine concern that his mind is sadly out of condition and that he really must do something to get it into order? Not he. It is a hundred to one that he will tranquilly accept the *status quo*, without shame and without very poignant regret."

Now what treatment is advocated by Mr. Bennett to bring about the best results so that a mind, that has shunted its tracks, so to speak, through overwork, may get back to the normal status. When a muscle is overworked, the owner of it immediately thinks of rest as the best means for restoring it to its best estate; but though one would think that Mr.

*New York: George H. Doran Company. 1911.

Bennett would not be averse from applying the same therapeutic measure to an overworked brain, that is really not what he wishes should be done. Rest, according to him, is not to be scoffed at; but it is not rest in the ordinary acceptation of the word, but the taking of the mind, from its forced, monotonous pabulum that has been overstocking each and every nook, to the quietness of concentration on one very small matter and this not for any great length of time. "Twenty lines a week for six months: what a 'cure' for debility! The chief, but not the only, merit of learning by heart as an exercise is that it compels the mind to concentrate. And the most important preliminary to self-development is the faculty of concentrating at will," writes Mr. Bennett; and really when we physicians descend from our medical clouds, so heavy with theories, will our sanity not prompt us to say that perhaps after all this mere literary man is right?

But, be it understood here, Mr. Bennett is not posing as a psychiatrist or even as a literary psychiatrist, if there is such a thing. What he really is interested in is for the man of ordinary intelligence to get out of the present—again the Englishman, for which he should be congratulated—all that can possibly be extracted from that auspicious period in our existence. And just because he feels that, on account of a mental inefficiency that was brought on by ignoring the right calisthenics, the ordinary individual is to-day not getting what should be coming to him, he wishes to preach his new gospel. That it is a good gospel, goes without saying; for, unlike our medical socialists, he does not blame the conditions under which we live but the individual.

LITERARY NOTES.

The making of popular dictionaries at low prices is quite an industry at the present time; and from the number which are published at short intervals one would be justified in concluding that the demand is ever on the increase. Just what element in our communities is beseeching for new dictionaries in one volume is not known to us; but, judging from the contents and general make-up of the books we have seen, it surely is an element that is easily pleased. But an altogether different matter in the making of a dictionary is "The Concise Oxford Dictionary" published for the small sum of one dollar by the Oxford University Press, New York. Here one will find none of those objectionable crudities which show a distressing lack of scholarship; none of those glaring faults as to letterpress which are due, not only to carelessness, but to indifference on the part of the editors. If a dictionary is really needed by a physician, who would rather have the contents in one volume of some 1,000 pages than in two or for that matter eleven volumes of, say, 10,000 pages, he cannot possibly go amiss as regards this adaptation from the more voluminous "Oxford Dictionary." Although in one volume, the definitions have not

been shortened, but are given with the fullness characteristic of larger works. Again all those idiomatic phrases without which a dictionary falls short as a reference-book are presented without curtailment as well as are the roots in various languages. For instance, the word operation is defined thus: Working, action, way thing works, efficacy, validity, scope, (*is in, comes into, o.; its o. is easily explained; must extend its o., make it valid for longer time or in wider sphere*); active process, activity, performance, discharge of function, (*the o. of breathing, thinking, pruning, etc.*); financial transaction; (Surg.) thing done with hand or instrument to some part of body to remedy deformity, injury, disease, pain, etc.; strategic movement of troops, ships, etc.; (Math.) subjection of number or quantity to process affecting its value or form, *e. g.* multiplication. [OF, f. L. *operationem* (OPERATE,-ION)] This rather lengthy quotation should suffice to prejudice the student for this excellent work.

As the title would indicate, Dr. J. B. Hurry's "Vicious Circles in Disease" (J. and A. Churchill, London) is not intended for the specialist who regards the subject of the interdependence of the organs of such secondary importance, that mention thereof is construed into gratuitous advice, if not an attempt to degrade his lofty position to the lowly one of general practitioner. Now though the message of this excellent work will fall on barren soil, as far as the hidebound specialist is concerned, its purpose cannot be misunderstood by him who is neither specialist nor general practitioner—according to the conventional interpretation of these words—but the product of so wide a knowledge of medicine that when he happens to be in general practice, the keenness of his eye is that of a specialist, and when a specialist, the thought of following the tactics of a general practitioner is not abhorrent to him. Books like Dr. Hurry's, while they may not be epoch-making, are really more than ordinary textbooks, for they adumbrate what is bound to come in medicine just so soon as the specialist and the general practitioner work toward one end—namely, a closer alliance so that the greatest benefit will accrue to the patient.

The appearance, at the present moment, of the authorized English translation from the fifteenth edition of Dieulafoy's "Manuel de Pathologie Interne," which in English bears the title "A Text-Book of Medicine" (D. Appleton, New York), is of greater significance than would at first appear; for the author of this masterpiece of French scholarship in medicine is no longer among the living, his death having taken place in Paris on August 16th. A splendid figure, indeed, was Dieulafoy: splendid—and our readers must pardon the use of this rather extravagant word—since he combined all those qualities which go to the making of an undeniable

force. Combined with an urbanity that was typically Parisian, he had the courage of his convictions; and his candor and outspokenness, when it was a question of combating theories which were distasteful to him, went far in pleasing those who admire these enviable qualities when courtesy is not entirely divorced from them. A pupil of Trousseau, he followed in the steps of this master; but his originality did not allow him to deteriorate into a mere imitator, but made of him what was probably the brightest star in the medical firmament of the Paris of to-day. The book before us needs no high words of commendation, for everyone who reads medicine with intelligence knows its full value. It is the textbook of textbooks; and though others with some claims to superiority may attempt to crowd it off our shelves, we imagine the effort will be futile. In a work that is so full of things that are above the average, the task of singling out any one chapter is indeed a difficult one; but, if we were called upon to give advice to the reader, we would refer him to the pages on Diseases of the Brain, Typhoid Diseases, and Infectious Diseases Proper to Man. Here we have Dieulafoy at his best: his lucidity, his grace as a writer, his sureness of touch, his supreme mastery of the subject. The English translation leaves nothing to be desired; and though those who have read the work in the original might cavil at the failure of the translators to put into English the Dieulafoy manner of writing, the criticism is unjust, since this would be asking what is wellnigh impossible.

A book that should not be overlooked by bibliophiles is "Francis Bacon" by Dr. G. Walter Steeves (Methuen and Company, London), for the reason that unlike most biographies we have here in concise form the outstanding events in the life of the great philosopher. In most biographies of our literary worthies, the author injects his own opinions to so great an extent that we really read an account of his mental habit instead of the salient points in the life of the literary character who is supposed to be the subject of the book. Now no such charge can be brought against Dr. Steeves' work, since his manner of writing is the opposite of the personal sort that is so enamored of its self-opinionated pose that there is a constant blurring of the pen-picture. The matter of Bacon, the man, is really of secondary importance in this "Life"; a fact which we are glad to record, since, especially with American readers, the interminable controversy about his leniency in allowing one, William Shakespeare, to filch his glory seems to be all that is worth remembering. While Dr. Steeves' "Life" is not dedicated to the cryptographic enthusiasts who have wearied our most pleasant hours with their visionary talk, it would have been a stroke of genius to have done so; for in their eagerness to learn a new cryptogram they would have devoured the book only to learn that Bacon can really stand foursquare without the assistance of Shakespeare's plays.

As the readers of this column no doubt know, our sympathies do not lie with those physicians who cannot resist the itch of writing poetry. Now though our strictures have invariably been severe, they were prompted, not by malice, but by the thought that some severity might stem the ever-rising tide of the ambitious in the medical profession, who think it beneath them to speak in mere prose. If we have failed in our endeavors, some rejoicing should be ours at the present moment, for the volume which has recently come to us from the Riverside Press, Cambridge, bears witness to the fact that once in a long while a physician can write poetry that is worthy of the name. In "Verses From the Southwest" by Dr. Theodore Clarkson Merrill, there are many lines that are above the average, and a few that declare him a poet of insight and of that elusive touch that has the power of raising ordinary words to enviable heights. Of all the poems in this slender volume, "Easter Lilies" has appealed to us most, for here we have simplicity, rhythm, and the matchless grouping of words that could not be effected by anyone but an adept in their values. In its lack of redundancy, its directness and the *mot juste*, this poem does not fall far behind Paul Verlaine at his best.

While not a brilliant book, "Recollections of an Irish Doctor" by the late Dr. Lombe Atthill (Religious Tract Society, London) will hold the reader's attention to good purpose if sincerity, truthfulness and honesty are still the enviable qualities of mankind. A plain, unvarnished tale this book is; and though it is shorn of those frills and furbelows with which the usual autobiographer dearly loves to adorn his retrospective glimpses—the frills, somehow, foregather invariably around the author himself—it makes just the sort of reading every physician should indulge in if he wishes to see his every-day existence mirrored in a book. But should the physician still cherish romantic ideas of what the practice of medicine will yield him, he would do well to turn to some other book; for here are set down with charming candor all the exasperating obstacles which must be overcome ere a physician can achieve success. The fight Lydgate in "Middlemarch" made against narrow provincial ideas, only to be worsted by them, has its parallel in Dr. Atthill's autobiography, except that in the case of the Irish physician a dogged persistence was finally rewarded with some show of success. We, who know Dublin as it is to-day, can hardly realize what it was some seventy years ago and how deplorably deficient it was in abetting the aspirations of a young physician. To get at a realization of the municipal conditions, the chapters in Dr. Atthill's book on "Dublin Seventy Years Ago" and "Slow Progress in Dublin" are illuminating; and he who is dissatisfied with his lot at the present day would do well to con them carefully, if only to learn that while at present the physician has small chance of success until he is thirty, in those far-off days it was really at a much later period and after years of waiting that Fortune deigned to smile upon him.

ORIGINAL ARTICLES.

THE ROLE OF THE MYOCARDIUM IN HEART DISEASE.

By LOUIS M. WARFIELD, M. D., of Milwaukee.

The more we have learned about the pathological physiology of the heart from the newer studies by means of venous pulse-tracings, cardiograms, and electrocardiograms, the more it is borne in upon us that the function of the heart muscle itself is what we wish to have most information about and what we know least about. Thus far we have no accurate means of measuring the actual power of the muscle, especially its ability to carry on the circulation against the odds of valvular disease, arterial disease, or disease of the muscle itself. We should like to be able to measure the tone of the muscle, but modern methods of research have yet to bring forward a means to do this with any degree of accuracy.

A normal heart muscle has five inherent properties: Irritability, rhythmicity, conductivity, contractility, and tonus. We are concerned with the two last, viz., contractility and tonus. By tonus we mean the force which resists over-dilatation of the muscle when the heart is in diastole. Normally the vagus nerve through the augmentor fibres controls this function of the heart muscle. When a normal heart is beating rapidly as the result of exercise, it, at first, dilates. If the exercise is not too severe, the transient dilatation soon passes away. The heart of the athlete or of one in training actually becomes smaller, that is, the tonus of the muscle increases following mild or even moderately severe exercise. The athlete's heart is often hypertrophied. It was formerly considered that there was no such condition as pure work hypertrophy, but experiments upon dogs which were made to run on a treadmill (Kuelbs) have shown that there is actually a true work hypertrophy.

It is extremely doubtful if the mere increase in the number of beats per minute could produce hypertrophy. Hypertrophy is generally conceded to result only after the muscle is stretched, and this takes place when the residual blood in the ventricle becomes greater than normal and acts as an increase in the load which the ventricle carries. Stewart (*Journ. Exper. Med.*, 1911, XIII, p. 187) produced such a condition in dogs by cutting the posterior cusp of the aortic valve with a MacCallum valvulotome. This caused a sudden insufficiency of the valve with a resulting excess of blood to the left ventricle at every diastole. To accom-

moderate this extra amount of blood the ventricle dilated. He found that the hypertrophy involved all the chambers of the heart with the greatest absolute and relative increase in the left ventricle. The auricles also hypertrophied to a relatively greater degree than the septum or the right ventricle. The co-hypertrophy of the auricles was not due to an altered venous pressure but resulted from increased force of the auricular systole. The heart showed increase in weight within one week after the production of the aortic insufficiency.

While a strong hypertrophied heart tends to decrease in size during exercise, the atrophic heart tends to increase in size or, in other words, to dilate. The auricles at first dilate enormously, later the ventricles. If, however, there is good tonus of the muscle, the ventricles in the normal heart do not dilate. A heart in which the muscle is diseased becomes dilated upon comparatively slight exertion.

Probably the most severe strain which one can place on a normal heart is the strain of lifting. The glottis is closed, the blood-pressure becomes greatly increased, the intrathoracic pressure rises, the veins of the head and neck stand out like cords, and excess of work is put upon the ventricle. In a healthy heart no harm is done. Exercise even to the point of exhaustion and fainting does not bring about cardiac dilatation in otherwise healthy men. Should the heart be diseased (fatty degeneration, myocarditis, etc.) not only dilatation, but permanent damage to the heart muscle may result from overstrain. In fatty degeneration of the heart there is diminished tonicity. This is one factor in producing permanent dilatation in such a heart.

The venous pressure is highest in exercise of strain and in straining at stool and during coitus. Frequently in the history of overstrain one or more of these factors are the first points noted. The high venous pressure keeps the heart dilated and the low tonicity of a diseased muscle permits the muscle to remain dilated. The most important point is repetition of the strain. Even a mildly diseased heart may recover from a considerable strain, provided the strain ceases at once or there is time for the heart to return to normal size before the advent of a second strain. But if the strain is repeated in a heart already dilated the tonicity is so low that permanent damage results. It would appear that the borderline between true heart failure and recovery from a strain depends upon the period of rest after strain.

The whole question boiled down to its last analysis is one of the integrity of the heart muscle itself. If the muscle is strong it will be able to stand enormous degrees of strain without becoming damaged. This becomes of greatest importance in the etiology of heart failure.

The conditions actually existing in the heart muscle and which cause it to dilate in response to overstrain and to remain dilated are not many. Pathologically there is parenchymatous degeneration due to the infectious fevers among which rheumatism and diphtheria stand pre-eminent. A

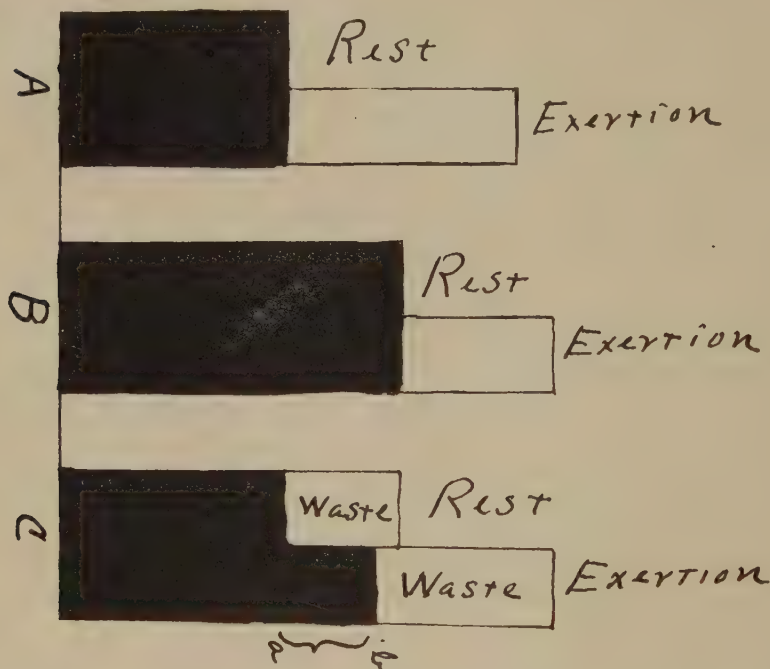
specific form of degeneration due to rheumatism has been described. Space will not permit of detailed description of the gross and microscopical appearances of cloudy swelling. Suffice it to say that it is the same in general as occurs in all the parenchymatous organs. The heart muscle necessarily, during the period of degeneration, is much weakened and subject to dilatation upon the least strain. Fatty infiltration is not a true disease of the heart muscle, but the fat may be deposited in such large amounts between the muscle-bundles as to produce pressure atrophy and replacement of some of the muscle cells by connective-tissue. This may not necessarily weaken the heart. On the contrary, it may be the means of adding strength to a heart which is embarrassed by too much fat. However, fatty degeneration is a true disease of the muscle characterized by destruction of the individual cells and replacement of the cells by fat droplets. A certain amount of connective-tissue also is deposited throughout the heart. A heart with this condition is weak, although it may do its work for a long time, never giving cause for suspicion that it is diseased until some slight strain is placed upon it.

Fibrous myocarditis is probably the commonest disease of the muscle, the one oftenest responsible for permanent dilatation. This may follow repeated strain as in chronic nephritis, arteriosclerosis, overeating, hard manual labor, etc. It has been shown that the hypertrophy which practically always is found in such hearts is first due to an imbibition of water by the individual cells. The cells also elongate but do not seem to increase in number. With the increase in size of individual fibres there goes hand in hand destruction of other fibres, and connective-tissue deposits take the place of the atrophied and destroyed cells. Some believe that continued strain leads to a replacement of the edema in the muscle by connective-tissue. Such changes have been found in the heart of experimental adrenalin myocarditis (Pearce, Fleischer and Loeb). It has been held that the deposition of fibrous tissue was due to anemia caused by contraction of the coronary arteries and consequent failure of the blood-supply. Fleischer and Loeb (*Arch. Int. Med.*, 1909, III, 78; 1910, VI, p. 427) in a study of experimental myocarditis produced by the injection of spartein sulphate and caffein sodium benzoate together with adrenalin, believe that the myocarditic changes in the heart are due to excessive contraction of the muscle-fibres of the heart and not due to the contraction of the coronary vessels. The lesions are almost invariably and always most advanced near the auriculoventricular ring in the left ventricle and in the tips of the papillary muscles where the strain is greatest. They also find that hearts showing such myocarditic changes on macroscopic examination were functionally inferior to normal hearts, when placed under conditions in which a surplus of work is required. This only bears out what has long been known clinically and proved at autopsy.

Finally, the heart may be weakened by actual atrophy of the organ due to decrease in size of the fibres and a degeneration of the cells in which granules of hematoïdin are deposited in a fusiform arrangement

around the nucleus. Normally there are a few granules at each pole of the nucleus. The heart so atrophied has a muddy brown color, the muscle is flabby, it is functionally exceedingly weak. The condition is known as "brown atrophy."

Weak hearts often develop peculiarities or rhythm. The inherent properties of the muscle are interfered with and we find intermittence, irregularity, and extra contractions. On the venous tracings are found all grades of irregularity up to fibrillation of the auricles. The pulse-tracings may show *pulsus alternans* and *pulsus irregularis perpetuus*. These occur only in severe permanent damage to the heart muscle. Extrasystoles are frequent. These mean damage to the muscle, although



Hirschfelder has observed a case of a young athletic man in whose pulse-tracings there were frequent extra-systoles. Fr. Mueller (Harvey Lecture 1906-1907), however, thinks that so frequently do we find heart failure or arteriosclerosis develop in later years in those who showed in their youth extra-systolic irregularity, that this probably was but a stage of evolution in those diseases and therefore is not insignificant.

Hearts, the subject of valvular lesions, are not necessarily weak hearts. So much stress is laid on valvular lesions during one's student days that one is apt to get the impression that a valvular lesion dooms the man, who has it, to an early grave. Lesions at the several valves naturally have different effects upon the functional power of the heart. And of

great importance is the distinction upon which Prof. Osler lays such stress, that of endocarditic origin, meaning the lesion following infectious diseases particularly rheumatism, and that of arteriosclerotic origin following progressive thickening of the valves and marked myocardial fibrous deposits. The prognosis in the former type is much better than in the latter type.

Valvular lesions in general only affect the heart in that they force the heart continually to do more work in order to maintain the circulation than the normal heart has to do. The normal heart has a wide range of flexibility and great reserve power. The heart with a valvular lesion is always encroaching upon its reserve power so that while it may actually be a more powerful heart, as in aortic insufficiency, its range of reserve power is really less than that of a healthy heart. This may be graphically shown in the accompanying chart.

The important point to bear in mind is that it is not the valvular lesion which determines the breaking down of the heart but the state of the heart muscle. It is not to be understood that hearts with valvular lesions are in any sense as strong as normal hearts. They are not as strong and they break down under strains which would not affect a normal heart. The very fact that there is a valvular lesion means almost invariably that the myocardium is more or less diseased, and it is upon that myocardium that the individual with the lesion in his heart has to depend for the functional power of his heart. The rôle of the myocardium then assumes the very first place in the pathology and pathological physiology of heart disease.

Valvular lesions are so obvious, as a rule, when the heart is examined that they focus attention upon themselves and one is apt to forget the fact that the muscle is also affected. They serve as a sign-post to warn of dangers ahead. Unfortunately, when only the myocardium is diseased, we have no means of knowing it until some accident occurs which causes symptoms of heart failure. Many a person is going around and doing his or her daily tasks with a much diseased heart muscle. We have no means of diagnosing it unless the person comes to us for some other complaint and we test out the muscular power of the heart under conditions which in a normal heart would produce no dilatation. Actual symptoms of heart failure are the first indications we have that an individual has a weak heart.

When the heart becomes unable to maintain the circulation regularly, one side or the other giving way under some extra load, there is stasis of blood. This condition is known as "broken compensation." This may be of two kinds, really of three kinds: (1) Broken compensation of the left ventricle; (2) of the right ventricle; (3) of both ventricles. Necessarily the symptoms vary with the side of the heart which fails. If the right side fail the symptoms reveal stasis in the systemic circulation. Should the left side fail to do its work the blood collects in the lungs and symptoms referable to those organs manifest themselves.

Broken compensation does not occur in a normal heart however severe the exercise may be. Even hearts with evident valvular lesions do not break down except temporarily, provided the muscle of the ventricles is not diseased. One may lay down the axiom that broken compensation is dependent upon excessive strain placed upon a heart whose muscle is the seat of pathological changes which have weakened the muscle.

The normal heart muscle is capable of becoming acutely dilated on frequent occasions. This fact is known to all who have ever had occasion to examine boys and young men following violent exercise. The pulse may be so rapid that it is impossible to count it. Nausea and vomiting may occur or the person may faint from the exertion. So far as we know such overstrained hearts return, after a time, to normal size by virtue of the tonicity of the heart muscle, and no permanent damage results. The essential difference between a heart with a normal musculature and one with diseased musculature must ever to be kept in mind.

We have as yet no means of diagnosing failure of the heart muscle until it actually fails. Time and time again are we surprised at autopsy to find the heart muscle so full of myocardial changes, macroscopically, and so many fibres ruptured and atrophic, microscopically, that we wonder how such hearts could have carried on the circulation competently. The explanation is that such a heart was not called upon to perform any work beyond its powers. We cannot diagnose myocarditis in such a heart except by reasoning from autopsy findings. Again there are mild grades of breathlessness on exertion and slight oppression beneath the precordial region, which are indications that the heart cannot carry even a small extra load without becoming dilated and causing some stagnation of blood. Severer grades show dyspnea, cough, pain over the liver, swelling of the feet, scanty urine, fluid in the serous cavities, etc. Examination shows that the heart is dilated, the rhythm is gallop, canter, or embryocardial in type. The rate is intermittent or irregular or both combined, and venous pulse-tracings may show a positive venous wave synchronous with the contraction of the right ventricle or a fibrillation of the auricles. Various means have been devised from time to time to measure myocardial changes. It is safe to say that the best authorities do not consider them of any great value. Reliance is based on symptoms which the patient shows after mild exercise rather than on measuring, with instruments, blood-pressure, pulse-curves, foot-pound power, etc. "The only true numerical criterion of cardiac efficiency is whether a given strain causes it to diminish in size (increase in tonicity=stimulation) or to dilate (decrease in tonicity=overstrain," Hirschfelder). The mechanism of heart failure is too subtle to be caught on graphic records. The sensations of the patient are much more valuable indications that the heart has failed to do its work.

From what has been said above, it becomes evident that the myocardium plays the chief rôle in disease of the heart. Valvular lesions mean that

the heart is working at a disadvantage, its reserve power is not as great, but it does not necessarily mean a weak heart. It seems to me that there is more than a fine distinction here. It means that every case of heart disease, that is, valvular heart disease must be studied carefully as a distinct entity and not as one of a group of cases having similar lesions. It is just here that there is the important difference between the really good physician and the average doctor. Habits of life such as eating, sleeping, mental work and worry, defecation, sexual excesses, drinking, and, in short, everything pertaining to the life of the patient must be most carefully and painstakingly inquired into before we can even begin to tell him what not to do. No detail is too unimportant for study. When we have grasped the fact that there are no unessentials in the history of a person with suspected heart failure, and the fact that a loud murmur does not necessarily mean real heart disease, and the other important fact that the state of the myocardium controls the situation, then we may have hopes of assisting many apparently hopeless invalids to useful lives.

PROLONGED PREGNANCY.

By ADAM H. WRIGHT, B. A., M. D., of Toronto,
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This short article is intended as a sequel to a paper on "Induction of Labour at Term as a Matter of Routine," published in the *American Journal of Obstetrics* two years ago. Since that time observation of cases, consultations, and conversations with fellow-practitioners have given me more definite opinions as to the evils of prolonged pregnancy.

The following recommendations were made in the paper referred to:—

Induce labor in all cases within two or three days after the expected date of confinement without waiting for any signs of labor.

First plug the vagina according to the Schauta method, making a special effort to pack the vault tightly.

— After packing allow the patient to get up and go about if she wishes.

Remove the tampon in twenty-four hours, introduce a new plug and again allow the patient to get up, and go about if she chooses.

Remove the second tampon in twenty-four hours after its introduction.

If by this time labor has not commenced it is generally advisable to pass a bougie into the uterine cavity before introducing the third tampon.

A few remarks will now be made in reply to criticisms and certain questions which have been asked. For the purposes of our present argument we shall consider the results in a pregnancy prolonged one month after term.

First, as to Child.—The growth of the child in utero after it becomes viable is very rapid. At the end of seven months the average weight is 1,400 gm.; at the end of eight months, 2,200 gm.; at the end of nine months, 3,470 gm., the increase in the latter month being nearly 58 per cent. The probable increase in the tenth month is 2,000 gm. In other words a child weighing seven pounds at term will weigh eleven at the end of another month. A child weighing nine pounds will weigh thirteen or fourteen pounds. In addition the child loses to some extent its flexibility; "universal flexion" is not so marked; worse still, the head becomes hard because of ossification.

We cannot speak so definitely as to the effects on the mother. We may say, however, that her nervous system is more or less seriously affected, she is frequently much depressed, and her general health is impaired in many ways.

The difficulties we meet in conducting the labor are always great—sometimes tremendous. The large ossified head neither moulds nor flexes properly.

In a large proportion of cases the occiput turns to the rear. The uterine contractions are unduly painful, and unsatisfactory in many ways. They commonly become weaker or cease altogether just at the time when the expulsive forces are required. Interference is generally necessary, and the results are frequently, if not mostly, disastrous for both mother and child.

Such evils are generally recognized, but many think that it is not right to subject the patient to the risk involved in the induction of labor unless grave dangers arise. The gravest danger is really the growth of the child in the uterus; and this we should not fail to appreciate, although we cannot see it. If the induction of labor is done in an aseptic way it is practically devoid of danger. In any case it involves much less danger than a labor when a pregnancy has been prolonged to ten months.

One of the objections raised is that we cannot always tell when the patient has reached "term." For instance, there may be evidence to show that conception has occurred, not shortly after the last menstrual period, but a little before the next period should have commenced. In such a case, or in any case of doubt, the accoucheur may obtain evidence by both external and internal examination which may assist him in arriving at a correct conclusion. If the matter still remains doubtful it may be well to wait for one week before inducing labor. It is safer, however, to induce labor one or two weeks before "term," than two weeks after.

Many questions have been asked about methods of procedure. The Schauta method of vaginal tamponade is not effectual in a large proportion of cases. Such being the case it seems better in the majority of cases to introduce a tube or bougie into the uterine cavity as the first step. The parts should be prepared as for vaginal hysterectomy. The patient is usually placed in the lithotomy position—"across the bed"—in private practice. A "weight" speculum is introduced, and the cervix is fixed with a tenaculum forceps. A sterilized gum elastic bougie (No. 12 English) is introduced gently within the uterus up to the fundus if possible, care being taken not to rupture the membranes. If membranes are accidentally ruptured, little harm will result if the vault of the vagina is at once tightly packed. Little, of Montreal, uses a medium-sized rectal tube with a flexible metal director; and finds this sufficient in all cases, while the smaller gum elastic bougie alone sometimes fails.

In my own practice I always use the vaginal tampon after the bougie is introduced. There is generally about an inch of the bougie in the vagina below the cervix. The patient is turned from the back to the Sims position, and the Sims speculum is introduced in such a way that the vagina is "ballooned." The gauze (medicated generally with iodoform) is introduced, and packed tightly over or round the small portion of the bougie projecting into the vagina. The bougie is quite flexible, and the portion below the cervix is generally turned at a right angle while the gauze is being introduced. The aim is to pack very tightly the vault and upper two-thirds of the distended vagina. If the lower third is tightly

packed it causes great pain, and frequently retention of urine. During the last few years I have used ordinary cheese cloth instead of gauze, for reasons which do not apply to our present argument. The material is cut into strips, four and a half inches wide and four yards long. One of these strips is usually sufficient for one packing; but occasionally a little less or a little more is used. This strip represents one half square yard, i. e., a piece of cheese cloth one yard long and half a yard wide.

The material (5 per cent. iodoform cheese cloth) is prepared as follows by Miss Margaret Lash, of Toronto:—

Take four yards of cheese cloth (good quality) twenty-seven inches wide. Tear (not cut) into strips four and a half inches wide and full length. Sterilize these strips and then boil in sterile water. Wring them as dry as possible (having hands covered by sterilized gloves) and thoroughly saturate them in the following preparation—

Eight ounces of a one per cent. solution of carbolic and sterilized water.

Enough castile soap to make a suds.

Three drams and one scruple of iodoform powder.

Mix thoroughly in sterilized basin with sterilized pestle or glass rod.

After thoroughly saturating strips, wring as dry as possible.

Pack gauze strips, one after another, into sterilized glass jars, and seal down while moist.

In some cases as before mentioned, the vaginal tamponade is sufficient to induce uterine contractions, and bring on labor. In all cases it appears to have a good effect in dilating and softening the vagina, the pelvic floor and the perineum.

I may say incidentally that in dry labors my custom now is to introduce the tampon as soon as possible after the rupture of the membranes. For such a purpose the cheese cloth is also better than gauze, because it is more likely to keep the liquor amnii, or a portion of it within the uterine cavity.

Another question frequently asked is this: Do you often find that introduction of the bougie, and especially of the tampon, produces pain? Yes, it frequently does, and I often desist, and get an assistant to administer an anesthetic—preferably ether. In such a case I always introduce both the bougie and the tampon.

Does protracted pregnancy frequently occur? So far as we can judge from the statistics of those who have investigated the matter it would appear that it occurs in about 15 per cent. of all pregnancies. Protraction to the extent of four weeks, or one month after term, probably occurs in 6 per cent. of the cases when there is no interference.

Is it still deemed advisable in all cases to induce labor, at or shortly after full term? As to this question, I shall speak now for myself alone. Careful observation and increased experience have fully confirmed my opinions expressed two years ago. I think, therefore, that it would be well, for both mother and child, to make it an ordinary matter of routine to induce labor in all cases within a few days after term.

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APPENDIX DYSPEPSIA.

By WILLIAM FITCH CHENEY, M. D., of San Francisco,
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Disturbance of digestion is an ancient story. Always it has played a large part in the sufferings of humanity; and to-day there is no more frequent complaint than "stomach trouble." Our fathers in medicine were inclined to blame the stomach for all disturbance of its function; and the laity still prove their own belief in this old doctrine by taking dyspepsia remedies for all their digestive ills. Old textbooks on medicine taught that the stomach was the proper point of attack, when the patient complained of "a want of appetite, squeamishness, sometimes a vomiting, sudden and transient distentions of the stomach, eructations of various kinds, heartburn, pains in the region of the stomach, and a bound belly," as Cullen wrote in 1787. Old dogmas die hard; but gradually a new doctrine is receiving acceptance—namely, disturbance of stomach-digestion is not always the stomach's fault. This teaching is comparatively recent. It was suggested many years ago, but it is only during the last decade that progress has been made toward its spread. Indeed, the past five years have seen more advance than any that preceded; and to-day medical science takes the position that dyspepsia is not an entity; that it simply presents a group of symptoms produced by numerous different causes; that its treatment must always be preceded by careful investigation of the whole body; and that frequently some other organ than the stomach is the real offender, even when the symptoms are wholly gastric. Furthermore, in any case of chronic, persistent dyspepsia it may also be taken for granted that functional disturbance is only a consequence of definite organic disease somewhere, and that there is no such thing as a primary gastric neurosis. This is the present-day teaching, and increasing experience seems to justify it.

Much dyspepsia is undoubtedly due to disease of the stomach itself—to ulcer, to cancer, to gastritis, to weak walls, to obstructed outlet, to inefficient glands. But all this is now an old story, well proved and generally accepted. A more recent chapter is that which tells of dyspepsia due to diseased gall-bladder or ducts; or to ulcer in the duodenum; or to some cause entirely outside the digestive tract, such as pernicious anemia. And now most recent of all, we have the assertion that dyspepsia is frequently due to chronic appendicitis. To be sure, this had been suggested by various individuals in different parts of the world—in France, in Germany and in America—previous to 1910. But it was in January, 1910, that

Moynihan* published his paper entitled "Remarks on Appendix Dyspepsia," which first called general attention to the subject. Moynihan claimed that the symptoms of both gastric and duodenal ulcer could be imitated, even to the production of hematemesis, where no pathological change was discoverable at operation except chronic inflammation of the appendix; that removal of the inflamed appendix was followed by cessation of all the symptoms, sometimes at once, sometimes only by degrees; and that no operation for supposed gastric or duodenal ulcer was complete until an examination had been made of the appendix. He called attention also to the fact that there are several types of appendix dyspepsia beside that resembling ulcer; explaining the gastric symptoms as due mainly to a protective pylorospasm.

During February, 1910, this paper called forth numerous letters, written to the journal that had published it, some of them condemning, some of them approving the new doctrine. Bowlby,** another English surgeon, objected that Moynihan's description of appendix dyspepsia was so vague that there were few people with flatulent dyspepsia who would not be included in it; that the symptoms described as characteristic were so diverse as to practically demand removal of the appendix for all dyspeptics; that Moynihan's case-reports were too few and the interval since his operations too short to justify conclusions as to final results after previous years of abdominal trouble. Sir Clifford Allbutt,† while expressing the "natural apprehensions of the physician lest there be nowadays some over-readiness in our surgical colleagues," felt it quite likely that Moynihan's views might explain some obscure cases of dyspepsia; and gave the history of 2 cases under his own observation, where gastric symptoms, long persistent, ceased permanently after removal of diseased and adherent appendix. Ewald†† called attention to the fact that as far back as 1899 he had described cases which he named "appendicitis larvata," in which dyspeptic complaints were due to diseased appendix. "In the course of years," he wrote, "I have observed quite a number of such cases in which I made the diagnosis, had the operation done, found my diagnosis confirmed and saw a prompt cure." W. Hale White,‡ while admitting the possible relation between diseased appendix and gastric symptoms, thought we should be slow to conclude that because a diseased appendix is associated with particular symptoms, it is the cause of them; and referred to cases under his observation where following removal of the appendix the dyspeptic symptoms remained uninfluenced or returned after a lapse of time as severely as ever. Other contributors of less prominence than those mentioned likewise discussed Moynihan's views in their letters, and numerous case-reports were thus brought out, most of them confirming the statements in the original paper.

**British Medical Journal*, January 29th, 1910.

***British Medical Journal*, February 5th, 1910.

†*British Medical Journal*, February 12th, 1910.

††*British Medical Journal*, February 12th, 1910.

‡*British Medical Journal*, February 19th, 1910.

In March, 1910, two more valuable papers on the subject were published in London, one by Paterson, a surgeon, and one by Fenwick, a physician. Paterson's article* was entitled "Appendicular Gastralgia; or the Appendix as a Cause of Gastric Symptoms." Like Moynihan he expressed the opinion that appendix disease may give rise to symptoms which closely mimic those of gastric and duodenal ulcer; and that latent appendicitis is a frequent cause of gastric hypersecretion and acid dyspepsia. He believed that the symptoms were the result of intestinal toxemia, due to intestinal stasis; or to a reflex influence of the diseased appendix upon gastric secretion. As a surgeon he laid down two important rules derived from his observations: (1) That no operation should be performed on the stomach except when a definite organic lesion of the stomach or duodenum was found, because gastro-enterostomy never cures appendicitis; and (2) that in all operations for supposed gastric or duodenal ulcer the condition of the appendix should be carefully investigated.

Fenwick's paper** was entitled "The Clinical Significance of Gastric Hypersecretion and its Connection with Latent Disease of the Appendix." He reported 112 cases operated upon for continuous gastric hypersecretion, which he divided into three groups: those due to ulcer or its scar on one or the other side of the pylorus; those due to gall-bladder disease; and those due to chronic appendicitis. In 22 cases out of the 112, disease existed in the appendix alone; in 5 there was coexistence of gastric ulcer and diseased appendix, and in 4 coexistence of duodenal ulcer and diseased appendix. In these latter cases Fenwick believes that the appendix disease is primary; that this induces the continuous flow of a hyperacid gastric juice; that this in turn excites severe inflammation of the stomach and is very liable to be followed by hemorrhagic erosions and ultimately by ulceration in stomach or in duodenum.

In the United States, as might be expected, the most valuable contribution on appendix dyspepsia has come from Mayos' clinic. In March, 1910, a paper by Christopher Graham and Donald Guthrie† was published on "The Dyspeptic Type of Chronic Appendicitis." This is based upon a review of 200 cases of chronic appendicitis in which stomach symptoms predominated. 115 of these had been operated upon a year or more before the report and their subsequent condition had been followed. 89 were found to be cured by the operation and their dyspepsia had never returned; 7 were very much improved; 13 were improved but still had occasional return of symptoms; while only 6 showed no improvement at all. In June, 1910, Munro of Boston,†† writing on "Dyspepsia and Indigestion Viewed from a Surgical Standpoint," also referred to the dyspepsias due to chronic appendicitis. His conclusion was "there is a type of dyspepsia most naturally ascribed to gastroduodenal ulcer,

**Lancet*, March 12th, 1910.

***Lancet*, March 12th, 1910.

†*Journal of the American Medical Association*, March 19th, 1910.

††*Boston Medical and Surgical Journal*, June 23rd, 1910.

but which is really secondary to an appendicitis and which is curable to a degree not yet definitely determined by simple appendectomy."

When we see how many different observers, working independently in different parts of the world, have thus reached similar conclusions regarding the existence of an appendix dyspepsia, we are forced to the belief that this condition must hereafter be reckoned with as one of the possibilities in all chronic disturbances of digestion. Several important questions at once present themselves for consideration. *First*, is there any typical clinical history from which it may be suspected that chronic appendicitis is the cause of the gastric symptoms? As a matter of course such clinical history must concern itself (a) with the digestive complaints and (b) with the evidence of appendix inflammation.

(a) As regards the dyspeptic history, it becomes clear from a study of the papers heretofore mentioned, that there is no one definite picture presented in all cases. Moynihan in his original paper called particular attention to the striking resemblance of many histories to that of gastric ulcer; the symptoms being pain soon after food, epigastric in location; flatulence and belching; sour eructations; nausea and frequent vomiting; even hematemesis at times. He says: "The typical gastric ulcer of the medical textbooks, in my view, is frequently the appendix." Paterson likewise calls attention to this resemblance between gastric ulcer and appendix dyspepsia. Graham frankly states: "In the early days many of these cases were operated upon by surgeons in general for ulcer of the stomach, and while no lesion was found in the stomach, a needless gastro-enterostomy was performed for the so-called medical ulcer, which operation brought the patient trouble rather than relief."

In other cases the history is that corresponding to hyperchlorhydria: persistent heartburn, water-brash, flatulence, nausea, but without pain or vomiting or hematemesis. This group is undoubtedly a common one; but as hyperchlorhydria does not often produce symptoms severe enough to lead the patient to submit to operation, the source of the trouble is not so frequently demonstrated as in the supposed cases of gastric or duodenal ulcer. It seems probable that many of the persistent cases of "sour stomach," resisting all forms of medical treatment, are due to chronic appendicitis with adhesions. In not a few instances this has already been proved by operation which revealed the appendix condition; by the cure of all symptoms following removal of the appendix, and by the return of the gastric secretions to normal after such operation.

The following case may be taken as fairly typical of this group of appendix dyspepsias. In April, 1909, a woman aged thirty-three sought advice for stomach trouble which she had had for two years past, worse recently than ever before. She had an abnormal appetite, was hungry constantly, and had a gnawing ache in the stomach whenever it became empty, particularly about four o'clock each morning. Taking food relieved her for a couple of hours, but then the pain returned and persisted

until the next meal. It was always most severe at night when there was no food in the stomach. She felt nauseated often but never vomited; and suffered much from bilious headaches. The bowels moved regularly each day. Excessive tenderness was found in the epigastrium on pressure and a succussion splash constantly, but no peristaltic wave, no dilatation, no evidence of food retention. The test-meal showed a total acidity of 56, practically all made up of free and combined HCl. Over the appendix region a small mass was palpable, the size of a hen's egg, but not particularly tender, freely movable, gurgling on pressure. After a year of diet and medical treatment the patient was no better. She was then persuaded to have the inflamed and adherent appendix removed, immediately thereafter lost her dyspepsia and it has never since recurred.

In the third group of cases the complaint is of heaviness and fullness and discomfort soon after eating, with flatulence and belching and frequent regurgitation of mouthfuls of food during digestion; and of inability to take as much food as desired, because the stomach soon feels overdistended. These cases may show a normal stomach analysis or a sub-acidity; and seem to be due not so much to secretory disturbance as to pylorospasm. Moynihan mentions such cases in his report. The writer has also observed one case of this sort. A woman, aged forty, complained she had had stomach trouble off and on for twelve years, but had been fairly well until a year before. Since then she had much bloating and fullness after food; also, while she had no pain after eating, within five minutes, no matter how much or how little was taken, the food began to come up in mouthfuls. This happened after every meal, but as a rule only a few mouthfuls came and then no more; though exceptionally this would go on for hours. It was not water-brash but solid food that was regurgitated, without nausea or any effort at vomiting. The bowels were persistently constipated. This patient never showed any evidence of organic disease in her stomach—no tumor, no tenderness, no dilatation, no food retention, no peristaltic wave; and though numerous analyses were made, the secretion was always practically normal. But at the first examination and all subsequent ones a mass was palpable in the appendix region, not tender to touch as a rule, but always causing discomfort when handled. Medical treatment of all sorts did no good. Even the rest cure, on the theory that the dyspepsia was neurotic, made no change whatever in the symptoms. Finally after several months the abdomen was opened and the appendix removed. It was found bound down and buried by adhesions throughout its entire length. Following this procedure the old symptoms at once disappeared; and though more than a year has now elapsed, the patient has remained perfectly well.

(b) As regards a history that definitely points to the appendix as the seat of disease, it is not always to be obtained. But previous vague attacks of abdominal disorder may have had more significance than was attributed to them at the time. Both Moynihan and Graham refer to the

importance of an early history of so-called "belly-aches" in childhood or youth, which were really appendix inflammation and laid the foundation for subsequent dyspepsia; and the so-called "bilious attacks" of later life, with headache, vomiting, languor, malaise, and sharp abdominal pain, set down as acute indigestion, Moynihan thinks are often really attacks of atypical appendicitis that leave adhesions or other obstruction of the lumen. Another point brought out both by Graham and Moynihan is that the pain in the epigastrium and the other symptoms of gastric indigestion in appendix dyspepsia are either excited or increased in severity by exertion or exercise, such as hard physical work, long walks, golf, or dancing. Such a history is contrary to that in ulcer or hyperchlorhydria, where exercise usually gives relief. Sometimes when no history whatever can be elicited of any previous acute abdominal attack, there is nevertheless complaint of occasional aching and soreness and tenderness in the appendix region for a day at a time, though never severe enough to put the patient to bed. Graham also recalls the often observed fact that after an acute attack of appendicitis, leading to prompt operation, the patient is frequently relieved of a long-standing dyspepsia whose cause was never previously understood.

Secondly, we have to ask whether there are any typical findings on physical or laboratory examination that aid in the recognition of appendix dyspepsia. Although the symptoms are entirely gastric, the physical examination gives none of the signs usually found in organic disease of the stomach, except epigastric tenderness, which may or may not be present. There is no tumor visible or palpable; no peristaltic wave indicative of pyloric obstruction; and no decided change in contour after inflation, such as is found in prolapse or dilatation. Investigation of stomach contents after a test-meal gives variable results. Hyperacidity is common; but on the other hand the acidity may be normal or sub-normal. Fenwick says that chronic hypersecretion is a frequent finding, as shown by the presence of hyperacid juice in considerable amounts in the fasting stomach in the early morning. But there is no food retention to indicate organic obstruction at the pylorus and no excess of mucus as in chronic inflammation.

With this comparative lack of evidence of organic stomach disease, there is on the other hand more or less evidence presented of trouble with the appendix. In the first place, there may be a palpable mass there, rolling freely under the examining hand, tender on pressure; or there may be simply extreme tenderness without palpable thickening; or when pressure is made at McBurney's point, the patient may complain only of pain in the epigastrium, or of epigastric distress and nausea. Even when chronic appendicitis is present, it is possible that no local signs may be elicited; or that they may be discovered at one examination and not at another; so that the failure to find them does not eliminate the presence of a diseased appendix.

Thirdly, the question naturally follows, Is it possible to so combine history and physical signs as to diagnose dyspepsia due to chronic appendicitis? Of the first importance is a careful clinical history; of past illnesses as well as of the present one, for so a clue may be obtained as to whether appendix attacks have preceded. In all digestive disturbance more is to be learned from the character and sequence of the symptoms than from physical examination; and time taken to collect and to arrange the details of the patient's story is time well spent. Surgeons who have seen appendix dyspepsia most frequently, as Moynihan, Paterson and the Mayos, all agree that the clinical picture most often resembles that of gastric ulcer. It is in cases of apparent gastric ulcer then, that this possibility should always be remembered and the appendix carefully examined before operation is done or before medical cure is instituted for ulcer. Even if treatment by rest and restricted diet removes all symptoms for the time, it does not follow that the pathology was in the stomach; for such treatment would also relieve the strain upon an inflamed and adherent appendix. There are points too in the clinical history of ulcer that ought to differentiate it from the other forms of chronic indigestion, if the case is carefully studied. Such are the long duration and the alternation of periods of distress with periods of comfort; the onset of the symptoms of pain, distress, sour eructations, nausea and vomiting one to four hours after food; and the relief afforded by taking food or drink or alkalies. Graham emphasizes these latter points particularly. He says it is not the chronicity of the indigestion that is peculiar in ulcer, it is not the degree or location of the pain, nor the occurrence of vomiting, gas, sour eructations or burning discomfort in the stomach; these details are common to many types of chronic dyspeptic trouble—gall-stones, appendicitis, cancer; but the characteristic point is the time the pain appears, with its accompanying symptoms of vomiting, gas and sour eructations; its regularity after meals; and its control by food, vomiting and alkalies. In chronic appendicitis, on the contrary, the time of onset of pain is irregular. It usually appears soon after food and often is present continuously, regardless of food, though in the epigastrium, and not over the appendix. There is not the definite regularity of symptoms after every meal; and nausea, distress, flatulence and distension are more common than severe pain. Finally, the symptoms in chronic appendicitis are not controlled by taking food, which usually disturbs rather than relieves; while vomiting is not so constant and does not give ease if it occurs; and alkalies have no effect at all. But after all is recalled about these fine points in differential diagnosis, it will be well to remember also the rule laid down by Paterson—"in all operations for supposed gastric ulcer, the condition of the appendix should be carefully investigated."

In his original paper Moynihan asserted that hematemesis might be due to chronic appendicitis, with no gastric ulcer present or other usual

pathology to account for it. Nine years previously W. Hale White* had published a paper referring to numerous instances not only in his own experience, but also in that of other British physicians and surgeons, in which a diagnosis of ulcer of the stomach had been made, in which hematemesis, gastric pain and vomiting had occurred, but in which no ulceration had been found at operation although the stomach was opened and search made; and he had asked the question, "Are not some patients said to be afflicted with gastric ulcer really suffering from a different disease?" Moynihan makes the statement that in perhaps a dozen of his cases there had been hemorrhage from the stomach in excess of a pint; where operation revealed no lesion whatever in the stomach or other pathology than that in the appendix. Paterson likewise admits the possibility of hematemesis and melena as symptoms entirely due to appendix dyspepsia; while Fenwick goes still further and claims that continuous gastric hypersecretion, whether induced by chronic appendicitis, by chronic gall-bladder disease or by the scar of a healed ulcer in stomach or duodenum, may cause minute hemorrhagic erosions, capable of producing alarming hematemesis, without any visible break in the continuity of the gastric mucous membrane. Other explanations offered to account for hematemesis in chronic appendicitis are sepsis, or embolism of an artery in the stomach wall, or toxic influences which we do not at present understand. However it may be caused, there is so much testimony accumulating as to its possibility in appendix dyspepsia that the occurrence of hematemesis can no longer be considered as speaking for gastric ulcer in the differential diagnosis between the two. As a corollary to this it follows that occult blood in the feces is also of no aid in differentiation, nor the Einhorn thread test; for tiny erosions, weeping blood, may give the feces reaction or discolor the thread, as well as an open ulcer.

One case of hematemesis apparently due to chronic appendicitis has already been observed in San Francisco. Dr. Chas. G. Levison** reported before the Cooper College Science Club, in April, 1910, the history of a young woman who throughout four years had several times vomited blood profusely. A diagnosis had been made of ulcer, because of apparently typical symptoms besides that of hematemesis and she had once had a course of diet for ulcer, but without cure. Finally Levison operated, finding no ulcer or other lesion of stomach or duodenum, but chronic appendicitis with adhesions, for which the appendix was removed. At once the dyspepsia and hematemesis ceased; and up to this date, as is learned from a personal communication, there has been no recurrence and the patient has remained perfectly well.

Fourthly and finally, how does removal of the appendix cure the gastric symptoms? It has been claimed by skeptics that the cure is the result simply of suggestion; that the dyspepsia is neurotic and that the removal

**Lancet*, June 29th, 1901.

***California State Medical Journal*, February, 1911.

of the appendix has not so much to do with ending the symptoms as has the psychological influence of the operation itself. This explanation of the cure has been discussed by Ewald, who refuses to accept it for the following reasons: (1) None of the patients coming under his observation were hysterical; they had become nervous from prolonged suffering, but could not be looked upon as being from the beginning hysterical or neurasthenic persons; (2) suggestive effects are never of long duration; they may be surprising but they do not last; while in his cases the cure was permanent, in some of them continuing after as much as ten years; (3) there are patients operated upon for typical attacks of appendicitis, who besides complain of pains not due to the appendix, and in such persons the latter complaints remain after operation, depending on coincident disease elsewhere.

It seems perfectly clear to those who have observed cases, whether few or many, that the removal of the dyspepsia is directly due to the removal of the appendix; and a rational explanation for this is not hard to find. The chronic inflammation in the appendix leads to adhesions that bind it down to the cecum; or to obstruction of its lumen so that its cavity contains concretions or thick, offensive, purulent fluid. The presence of these abnormalities leads reflexly to protective spasm of the pylorus or to a continuous hypersecretion of gastric juice; and the removal of the cause produces a cessation of the effects. Within the last few years physiologists have proved conclusively that the various parts of the gastro-intestinal tract are mutually dependent one upon the other; and it is not surprising that disease of one part such as the appendix, should have an influence upon another such as the stomach. Let us then have faith when reason gives so good a basis for it, and in the future let us not hesitate to diagnose appendix dyspepsia when the evidence warrants, or to advise operation when diagnosis is made.

FURTHER OBSERVATIONS ON HIGH FREQUENCY CAUTERIZATION OF BLADDER LESIONS.*

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Since the publication of a preliminary paper upon the treatment of bladder tumors by the high frequency current a year ago, it has been found that the single pole, or Oudin current, is far more efficacious in the treatment of these lesions than the D'Arsonval current, which I was using at that time. The superior efficacy of the Oudin current is due to the greater convenience of its applications in the bladder, and, perhaps, also, to a superior electro-therapeutic quantity. The precise mode of action of these currents is nevertheless still so much a matter of dispute that it seems preferable, as it is certainly practicable, to discuss the matter as though the electricity produced a simple burn.**

Technique.—The apparatus required is a cystoscope with ureter catheterizing connections, also an electric generator for the production of the high frequency current, and an insulated wire sufficiently small to pass through the ureter catheter channel.

The instruments which I have employed are made by the Wappler Company. Judging from personal experiments with other apparatus, and from experiences related by others, it is evident that certain instruments for the production of high frequency currents are not suited to this work. This failure is said to be due to the relatively high voltage as compared with the amperage, and to relatively slow oscillation of the current.

The insulated wire is cut short off at the end which is to be introduced into the bladder. From the opposite end the insulation is removed to a sufficient extent to make the necessary connections with the generator. It is my usual practice, for reasons to be explained later, to employ a cystoscope with channels for two ureter catheters, introducing a wire into each. The current to be employed should be produced with almost the smallest possible spark gap at the generator. A large spark gap, by increasing the voltage, causes unnecessary pain and excites within the bladder so violent a destruction of the epithelium in contact with the wire as to obscure the field unnecessarily by the production of epithelial detritus and hemorrhage. Moreover, it shortens the duration of the operation by rapidly burning off the insulation of the wire.

The patient is prepared for cystoscopy in the usual way, the cystoscope

*Read before the Twenty-fifth Annual Meeting of the American Association of Genito-Urinary Surgeons, New York, June 2nd, 1911.

The case reports have been completed to July 1st, 1911.

**Beer (*Annals of Surgery*, August, 1911) has fully discussed this part of the subject.

introduced, the tumor inspected. Then one of the wires is projected against the tumor and pushed into it. In this manipulation the end of the wire may be lost sight of among the villi of the growth, but this is a matter of no moment. The current is then turned on for a brief space and the patient asked whether he feels it. If the wire has slipped from among the villi and come in contact with the bladder wall, or if there is a short circuit anywhere, the patient will feel pain; but if the wire is solely in contact with the tumor no pain is felt.

The current is then turned on and if the point of contact between the wire and the tumor has remained in the field of vision, one sees a rapid blanching of the tissues at that point, accompanied by a throwing off of what appears to be epithelial dust and many bubbles; and if the cystoscopic illumination is turned off one can often see a glow or even a spark at this point of contact. In case the end of the wire has disappeared from the field of vision, the fact that the current is working may sometimes be established by turning off the electric light and looking for the glow that arises from the spark at the end of the wire.

The duration of the burning is limited by several factors. In the first place, by the patience of the patient, or, if this is very great, by that of the operator. In the second place, by the fear of burning the bladder wall, when only a small bit of the tumor remains to be removed. In the third place, by the destruction of the insulation of the wire itself, for the heat of the current burns the rubber and if this goes on too long, the operation is brought to a close by leakage of electricity either through the water in the bladder or by short circuiting to the cystoscope. If a long spark gap is employed this short circuiting may burn out the cystoscopic lamp, but with a properly adjusted spark gap I have not had this accident.

A further difficulty due to burning off of the insulation is the fact that withdrawal of the wire may peel off a piece of the insulation, which drops into the bladder and may prove a nucleus for stone. This may be avoided by carefully lowering the lever (when an indirect vision cystoscope is used) and withdrawing the wire and telescope together, as one piece. The loose insulation will then fall naturally into the cavity at the end of the telescope and will be withdrawn with the wire. This precaution is only necessary when the cauterization has been prolonged and the wire is out of sight, so that its condition cannot be ascertained. Even if the insulating rubber does fall from the wire, a bladder that empties itself may be depended upon to eject it spontaneously.

In order to avoid all these difficulties, it is preferable to use the wire with the strongest insulation (this is one made of multiple strands of copper), and to make each burn of no more than thirty to sixty seconds' duration. After this an interval of a few seconds will cool the wire somewhat, when the cauterization may be repeated in the same spot or in a different spot. If the tumor does not bleed and is in plain view, one can thus go from

place to place on its surface until the whole of this has been reduced to a white mass with charred spots upon it where the actual contact with the wire has taken place. If the insulation of the wire softens during this process, one shifts to the wire in the opposite ureter catheter channel. When manipulating difficult tumors, i. e., those that bleed, or that overhang the urethral orifice, or that fall between the end of the telescope and the wire, the utmost dexterity may be required to obtain satisfactory results. In such cases one may sometimes derive much help by pushing the tumor away with one of the wires and then burning it with the other. In such instances, also, a systematic covering of the ground is quite impossible. One must burn more or less haphazard.

The frequency with which one may safely repeat these burns depends chiefly upon the condition of the patient. Inasmuch as the operation usually has to be repeated several times, it is well to make every effort at the time of the first cystoscopy to obtain very satisfactory anesthesia and to acquire the patient's confidence by stopping the burning or withdrawing the cystoscope at his slightest request.

I have usually repeated the burnings at intervals of two weeks, for the reason that almost all my cases have been charity cases and I have not been able to spare the time for a more frequent application. I have found, however, that for most cases an interval of a week is ideal. Twice a week is rather frequent for a cystoscopy, and at the end of seven days the line of demarcation between a burned spot and an unburned spot is sufficiently clear to show one where to make one's next burn. Complete detachment of the slough takes from two to four weeks.

The treatment has produced only the following accidents:—

- (1) Detachment of the insulation.
- (2) A single case of very severe hemorrhage which subsided after two days' rest in bed, without any other treatment, and which occurred a week after a rather prolonged burn.

It is the rule that no hemorrhage occurs after the first burning with the exception of such slight bleeding as is excited by the application itself. Occasionally slight hemorrhages have appeared some days after the burning, but with the one exception noted, they have been entirely unimportant, exception made for the infiltrating carcinomata.

- (3) A third condition, which can scarcely be called a complication (and which has been noted by both Dr. Beer and by Dr. McCarthy) and was depicted in the illustrations of my preliminary paper last year, is a curious reaction of the bladder wall to the irritation of the current. The mucosa swells up in such a way as to simulate an infiltrating carcinoma. Several weeks' intermission in the burning will suffice for the subsidence of this.

Effects of the Treatment.—The results obtained from this treatment in papilloma of the bladder have been most gratifying. The effect of the current upon infiltrating carcinoma is apparently nil. I have tried the

treatment by the method suggested by Dr. Beer upon two cases of prostatic hypertrophy with no result. Three cases of bladder ulceration have also given unsatisfactory results (one improved, two unimproved). I have been rather disappointed in the effect of this method of treating the engorged verumontanum.

In considering the details of my results which follow, pray bear in mind that until within the last month my source of electric current has been an ancient machine which is manifestly far inferior to the one I am at present using, and, furthermore, a great many of the treatments have been given partly by way of demonstration, which interferes greatly with the thoroughness of one's work, so that with a modern machine and no distractions one may expect to obtain cures very much more rapidly.

I have treated by this method every case of papillary tumor seen by me since October, 1909. There were twelve papillary tumors, three of them multiple. Of these cases, two are still under treatment; one was turned over to another physician for the completion of the treatment (he having loaned the case, as it were, up to the time that he obtained an instrument himself). One other eloped after three treatments.* The others, eight in all, with thirteen tumors, have been cured. The cures of individual tumors have been verified nineteen months after (Case I); one year after, two tumors (Case V); ten months after (Case VI); nine months after (Case II); seven months (two tumors in Case II, and one in Case VII). Thus three cases have been verified a year or more, and five others from seven to ten months. On the other hand, two very small tumors (Case IV) that were burned but once were found by another physician to have relapsed nine months after this burning.

The duration of cures in these cases is brief, to be sure, yet sufficient to justify the hope that every papillary bladder tumor should be submitted to this form of treatment before operation is contemplated. It is, of course, impossible to distinguish the presence or absence of induration in the base of a papillary tumor seen through the cystoscope, and many of the tumors treated by me have suggested carcinoma; while in one (Case II), the diagnosis of carcinoma was made by a pathological examination although the tumor looked like a small papilloma. The question of malignancy of bladder growths is one that cannot be discussed here.

We have classed all our cases as papillomata although, no doubt, many of them would be classified pathologically as carcinoma. On the other hand, several cases of bladder ulceration by neoplasm with infiltration of the bladder wall, palpable by rectum, seemed to be benefited in no way by this form of treatment.

CASE REPORTS (PAPILLOMATA).

Case I.—Male, forty-four years of age, had hematuria intermittently for fourteen months. I cystoscoped him in July, 1909, and discovered a large papilloma on the right side of the bladder and subsequently removed this through

*She has since reported herself well, but refuses cystoscopy to prove this.

a suprapubic incision. In September a relapse was noted by cystoscope. He was burned with the D'Arsonval current five times, the last time on November 6th, 1909. Since that time he has been cystoscoped on a number of occasions, the last time on June 1st, 1911, and no evidence of relapse has been seen.

Case II.—Male, sixty-one years old, hematuria intermittently for two months when first seen in August, 1909. A small tumor was seen near the right ureteral orifice, and this was removed through a suprapubic incision. In November the cystoscope revealed a small relapse in the scar; meanwhile a pathologist had reported carcinoma, but clinically the tumor always remained benign. The tumor was burned four times, the last time on April 23rd, 1910. When last cystoscoped, on January 28th, 1911, no relapse had occurred in the scar, but in May, 1910, three small villous tufts had appeared near the vault of the bladder on the lateral wall. I destroyed two of these by one burn as verified seven months later. The third I was uncertain of and did not burn until January 7th, 1911 and once thereafter. On October 5th, 1910, a small villous growth was seen on the upper wall of the urethra at the entrance to the bladder. I burned this and burned it again twice in January. Since then the patient has refused to be cystoscoped and has had no further symptoms.

Case III.—Male, aged seventy-eight years. Has had hematuria intermittently for eight years. Cystoscope shows bladder so full of tumor and bleeding so readily that satisfactory examination is impossible. He was burned once with the D'Arsonval current and ten times with the Oudin. Two weeks after the last burn he was reported by his physician to be dying of uremia.

This patient was reported by me as having died, but he turned up again six months later with the statement that his bleedings had been very slight during that time but had again become continuous for the last two weeks. He had received three more burnings during the month of June, but his bladder is still so full of papilloma that I am still unable to state whether the tumor is single or multiple.

Case IV.—Male, aged thirty-three; intermittent hematuria one year. A sister died of cancer of the bladder. Cystoscope revealed four tumors, a large one just to the left of the left ureter orifice, two small tufts along the urethral-vesical ring, and another small tuft in the vault. The large growth was burned twice with the D'Arsonval and six times with the Oudin, at the end of which time it was almost destroyed, and the case was turned over to the physician who had sent him, and who has since completed the cure. Meanwhile I apparently destroyed the three small tumors with one burn each, but two of them relapsed nine months later and were destroyed by further cauterization.

Case V.—Female, aged sixty. Single hematuria. Rather small tumor which ultimately proved to have a double base, growing from the left side of the bladder just beyond the ureter orifice. Burned once with the D'Arsonval and four times with the Oudin current, the last time June 16th, 1910. Cystoscope shows that she remains healed, June 1st, 1911.

Case VI.—Female, aged twenty-five. Hematuria and urethrorrhagia constantly for one week. Cystoscope reveals a tumor two cm. in length behind the left ureter. Burned twice with the Oudin current. Last burn, June 3rd, 1910, and cure verified by cystoscope April 12th, 1911.

Case VII.—Male, aged thirty-seven; tubercular hip since youth. For eleven years has urinated frequently and during this time has had six or seven hemorrhages. Urine very purulent. Cystoscope reveals an enormous tumor growing from the bladder neck and very difficult to burn owing to bleeding and because it falls so close against the cystoscope. Burned with the Oudin current 12 times, the last burn in November, 1910, at which time the bladder was much inflamed, but apparently free from tumor. The cure was verified by cystoscopy on June 8th, 1911.

Case VIII.—Male, aged forty-nine. Frequency of urination one year. Hematuria eight months. Cystoscope shows a large papilloma filling the right side of the bladder and extending beyond the middle line both above and below. Burned fifteen times. The tumor has apparently been cured, but cure has not been verified.

Case IX.—Female, aged twenty. Frequent and painful urination and pus in the urine for one year. No hematuria. Cystoscope reveals many papillæ growing from the trigone. The tumor was burned three times and the patient then eloped. (See text.)

Case X.—Male, aged forty-six. Intermittent hematuria for fifteen years. Cystoscopy shows a large papilloma to the right of the trigone; burned six times, the last time June 12th, 1911. Cure not yet verified.

Case XI.—Male, aged fifty-two; hematuria two months, intermittent and profuse. Cystoscope shows a bleeding papilloma just back of the left ureter. A single burn on June 16th, 1911, almost completely destroyed the tumor as was verified by an examination a week later, at which time a single papillary tuft was burned. Cure not verified.

Case XII.—Male, aged fifty. Profuse hemorrhage one month. Cystoscope revealed a bleeding papilloma just behind the trigone. Burned June 27th, 1911.

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METHODS OF DEALING WITH THE SAC IN OPERATIONS
FOR THE RADICAL CURE OF INGUINAL
AND FEMORAL HERNIÆ.

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There is reason to believe that the ordinary indirect inguinal hernia is always to this extent congenital, that a sac lined with peritoneum persists after the descent of the testicle is accomplished. This was the opinion of R. Hamilton Russell,¹ R. W. Murray,² Wm. Sheen,³ W. T. Bull⁴ and others; and while their theory has met with some opposition, it is coming into more and more universal favor. It is not intended here to review their arguments. The paper of Mr. Russell in particular is so convincing and the evidence adduced against him so very feeble that most of us will be content to regard the discussion as closed.

Let us grant then that the sac of an inguinal hernia exists in all cases from birth and awaits only some extraordinary exertion or some relaxed condition of the parts to receive a loop of bowel from above. This then is the accident which calls the patient's attention to his condition and brings him to the surgeon. In dealing with hernia, therefore, the obliteration of this sac must always be insisted upon as the one all important step in the operation, and indeed some have even maintained that, if this is successfully accomplished, all reconstructive procedures, all reinforcing of the abdominal wall and narrowing of the rings by means of sutures, may be considered superfluous as the bowel will not come down when there is no sac to receive it.

Acting upon this assumption Russell reports that in operating upon children he limits his procedures always to the simple removal of the sac; and in a series of 143 cases has not experienced a greater number of relapses than occurs in the practice of his colleagues who employ a more elaborate technique. In adults the conditions are slightly different as the tissues have now assumed their permanent character and relations, and the rings are generally somewhat dilated from the long continuation of the hernia. Therefore, it would seem wise to attempt some reconstructive operation, and few are likely to advocate the employment of Russell's technique in dealing with older patients. Kocher,⁵ however, comes very near to doing so and careful examination of his method may even convince us that he practically does do so. Kocher exposes the fascia of the external oblique but does not split it. He then frees the sac from the cord at the external ring; grasps its free end in a long pair of

forceps and inverts it, pushing the forceps along the canal as far as the internal ring. Here he nicks the aponeurosis of the external oblique with a knife and delivers to view the point of his forceps carrying the end of the sac. The sac is now drawn forcibly forward, tied off as high up as possible and removed. At this point, however, Kocher appears to have some misgivings as to the sufficiency of so simple a procedure; for he now tightens the aponeurosis of the external oblique by means of a single row of sutures each including about an inch in its grasp, so that when these sutures are tied this inch is drawn together and laid down as a dense fascial mass over the canal. It is difficult to see just what advantage this offers over an initial splitting of the aponeurosis, but Kocher claims a permanent cure in 92.5 per cent. of all cases and will allow the second best method (the Bassini) but 89.8 per cent. of enduring results.

Kocher is loud in his claims for the simplicity and reliability of his method and its infliction of a minimum of trauma upon the tissues. It has, however, one great fault. It is a compromise. If the simple removal of the sac is sufficient for a cure, why pass a row of sutures through the intact external oblique? or if sutures are to be passed through it, what good reason is there why it should not be split to begin with and a view obtained of the subjacent structures? Then if the conjoined tendon is found widely retracted, as it is likely to be in many herniæ of long standing, especially where a heavy truss has been worn, it may be brought down and sewed to Poupart's ligament either above or below the cord as seems best at the time, and how this can militate against the permanency of the result it is indeed difficult to understand. It must, however, be admitted that bad technique may here give rise to a strangulation of the cord and that the danger of infection consequent upon increased trauma is somewhat greater than under the Kocher method.

However, the point to be emphasized is the tremendous importance of the sac in the etiology and pathology of inguinal hernia and the success of those operators who rely mainly upon its complete extirpation for cure.

Of femoral hernia much the same is to be said. Russell is uncompromising. He asserts that the sac in all cases of femoral hernia is a congenital anomaly. Murray supports him. Their arguments and their array of evidence are very convincing, and so far as I have been able to discover nothing of much weight has ever been brought forward to oppose them. E. M. Corner⁶ has something to say on the other side, but appears to us to have been successfully answered. And, if the sac here plays the same rôle as in the case of inguinal hernia, all will agree that its complete removal must here also be the all-important step in procedures looking to a cure.

Bearing the above in mind it is somewhat surprising to discover how little has been written on the subject of dealing with the sac in operations

both for inguinal and femoral hernia. A careful review of the French, German and English literature of the past five years reveals but three or four papers which bestow any considerable amount of attention upon it. Confining ourselves for the present to inguinal hernia, we find that surgeons have vied with each other in devising ingenious modifications of the Bassini operation and other similar procedures, but in their zeal for reconstructing the canal and strengthening the anatomy of the parts have diverted their attention somewhat from what is after all the crucial point—namely, the care of the sac. Many, probably most, content themselves with simple removal after firm ligation, leaving the ligated neck free at the internal ring. This is exactly what Kocher does, and the impossibility of any other mode of disposition under his technique is another objection to his method. Bull appears to have done the same, as also does Russell, and indeed it seems a safe assumption that ninety per cent. of all surgeons follow a similar practice. And, yet, is it not obvious that this ligated neck forms a point of least resistance in the peritoneum? Scar-tissue is going to form here and scar-tissue is weak tissue. But a more important consideration is the following. What surgeon can be absolutely sure that he has always applied his ligature at the very uppermost limit of this peritoneal process which we call the sac, and has not left still half an inch or so which remains above as a little pouch, or at any rate a relaxed area of peritoneum—a potential pouch, ready to become a pouch as soon as pressure is exerted upon it from within? Recurrences after operation do occur and, though we possess no final information as to how the new sac is formed, it would nevertheless appear highly undesirable to leave this point of least resistance in the peritoneal investment of the abdomen to press directly against the orifice of the internal ring inviting the abdominal pressure to force it through. How much more reasonable it would appear to draw it upward and fix it securely behind the heavy muscles of the abdominal wall. Here at least it would be out of mischief, virgin peritoneum would slide up to take its place over the orifice of the internal ring, and should our hernia recur, it would at any rate have to form an entirely new sac for itself of which it would find not even the rudiments prepared.

The point is not a new one. Macewen appears to have perceived it when he devised his operation, but his method is difficult and dangerous. DaCosta⁷ describes it as follows: "The bowel is reduced and an incision three inches long is made in the direction of the inguinal canal, the centre of the incision corresponding to the external ring. The sac is freed from its attachments below and is lifted up. The surgeon introduces a finger into the inguinal canal and separates the sac from the cord and from the walls of the canal, and then carries the finger through the internal ring and separates the peritoneum for one inch about the periphery of this aperture. A chromicized catgut stitch is fastened to the lowest portion of the sac and is passed through the sac several times so that pulling

on the stitch will purse the sac. The free end of this stitch is passed through the internal ring into the belly, and is pushed out through the abdominal muscles one inch above the internal ring, the skin being pushed aside so as to escape perforation by the needle. The thread is tightened so as to fold up the sac and pull it into the belly. This plugs the ring. The sac is permanently anchored by taking several stitches in the external oblique muscle." This reads as if the aponeurosis of the external oblique were not to be divided, but the successful carrying out of the procedure would certainly involve such division, and a little further on we read that the conjoined tendon is to be sewed to Poupart's ligament, which

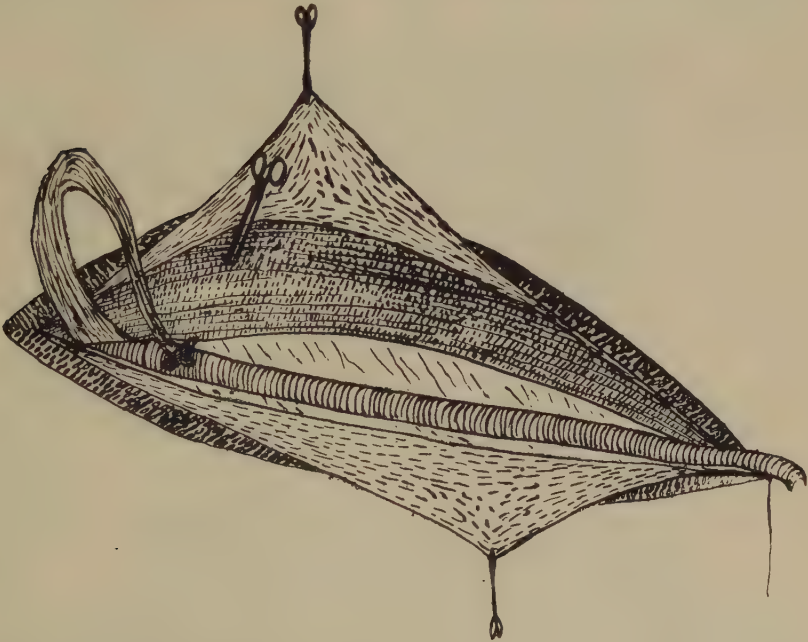


Fig. 1.—Lexer's method of dealing with the sac in the operation for the radical cure of inguinal hernia.

could certainly never be accomplished if the external oblique remained intact. But, granted that the internal ring was fully exposed, what are we to say of the introduction of a needle through it into the abdominal cavity and the pushing of this needle forward an inch above through the abdominal wall, guided only by the finger? That this could be successfully accomplished no one will question, but most surgeons will certainly regard it as an unnecessarily awkward procedure. We like to work openly where we can see what we are doing, and manœuvres carried out through very narrow orifices in dark cavities must always have their attendant dangers. As for the plugging of the ring with the redundant portion of the sac, few outside of its originator have ever appeared to

defend it. If the ring is too large we can narrow it with a stitch down to any size he may choose, and there can therefore be no excuse for plugging it with tissue that is here foreign and likely to become necrotic. Macewen has had few followers, and his leaving of the sac after strangulating it with his suture may be discarded, but he does deserve credit for his idea of pulling the neck of the sac upward away from the mouth of the internal ring; and it is certainly to this feature that his operation owes any success which it may have achieved.

It remained for Lexer⁸ to give us a method whereby the same result can be accomplished without the necessity of invading the peritoneal cavity, and by a method which is simple, effective, and free from danger. He divides the skin and the aponeurosis of the external oblique in the usual way and frees the sac as far up as the internal ring where he ligates it securely as high up as possible, but does not yet remove it. He now passes a pair of slightly curved forceps under the free margin of the conjoined tendon, insinuating them gently upward between the muscle and the peritoneum for a distance of about two inches. Here he pushes the point of the forceps forward through the muscle. Into the jaws of this forceps he now introduces the jaws of a second pair of similar forceps, locks them securely and withdraws the first pair, thus conducting the second pair along the route of the first down toward the internal ring.

The situation is now that shown in the illustration. The loose end of the sac is now clasped in the jaws of the forceps which have been thus placed, and the forceps withdrawn. This brings the sac out through the muscular tissue at a point about two inches above the internal ring. It is pulled upon until the neck of the sac or point of original ligature comes to lie firmly against the posterior surface of the muscle at this point, a thing which requires no great amount of force as the peritoneum is but loosely attached in this region and slides upward very easily. Two or three sutures now anchor the sac to the muscle and the redundant portion of the sac is cut away. The Bassini operation may now be performed or any other procedure resorted to which the requirements of the case may seem to indicate.

This procedure appears to offer all that can be required of an ideal method of eliminating the sac. It is done entirely extraperitoneally and is so simple that it adds practically nothing to the difficulty of an ordinary herniotomy. One performing the operation in this way for the first time is surprised at the ease with which the ligated neck slides up to its new position. There seems to be no resistance whatever, and, should it occur to anyone that a possibility of tearing the peritoneum may lurk as a remote danger, he has but to try the method once to dispel his misgivings. Indeed, the first resistance encountered is the absolute one arising when our point of ligature impinges against the posterior surface of the muscles, and indeed it is this resistance which tells the surgeon that he has accomplished his purpose and may proceed with his sutures. The time required is but an additional minute at most.

The amount of laceration inflicted upon the muscle, by the thrusting of a pair of forceps through it, is of course negligible, and even granted that a portion of the sac remains penetrating the muscle, I think no one will see any danger from this source especially when it is considered that the aponeurosis of the external oblique, which has been drawn upward during our manœuvre, falls when released completely over the area involved. Should the surgeon however wish to avoid the drawing of the sac through the muscular tissue, he may proceed somewhat differently as I have seen done by Dr. W. T. Coughlin, of St. Louis. Having ligated the sac he leaves the end of his ligatures long and threads each upon a needle. He then removes the sac. He passes an additional suture

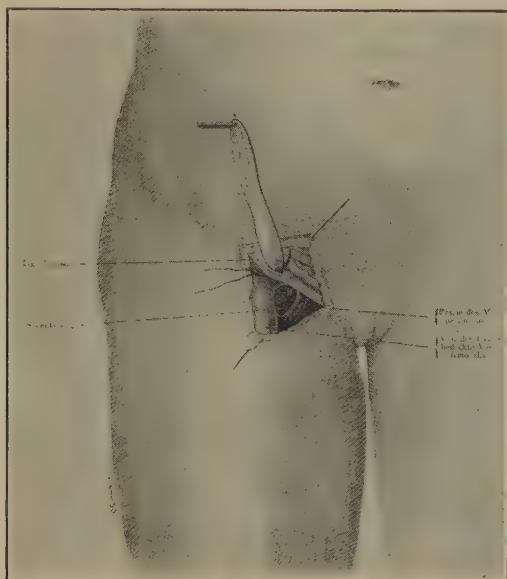


Fig. 2.—Kocher's method of dealing with the sac in the operation for the radical cure of femoral hernia. The needle is shown passing through Poupart's ligament which would anchor the neck of the sac in its new position. His illustration shows this technique, but he expressly repudiates it in his text, saying that this needle should pass through the sac alone. We would suggest the propriety of performing the operation as shown above.

through the neck of the sac, and again threads each end upon a needle. Now he inserts a finger under the free margin of the conjoined tendon and dissects it bluntly from the peritoneum for a distance of about two inches, at which point he passes his needles through the muscular tissue from within outward, in such a manner that his knots when tied shall lie in a direction parallel with the muscular fibres and about a centimetre and a half apart. The tying of these knots now draws the neck of the sac firmly up against the posterior surface of the muscles precisely as is done when the Lexer method is employed.

Papavannou⁹ suggests a similar procedure. He likewise, having ligated and removed the sac, leaves the ends of his ligature long and threads each upon a needle. He then separates the conjoined tendon from Poupart's ligament for a distance of an inch or more above the internal ring in the direction of the anterior superior spine of the ileum. At this point he passes one needle through the muscle and the other through Poupart's ligament. When the two ends of the suture are now drawn tight, the neck of the sac is pulled away from the mouth of the internal ring for a distance of about an inch outward along Poupart's ligament. A firm knot completes the technique.

This would appear to be a less desirable disposition than that of Lexer, for the reason that it leaves the neck of the sac in a somewhat weaker position. It is probably, however, sufficiently secure.

In dealing with femoral hernia, the principles are the same. The neck of the sac should not be left at the inner orifice of the femoral canal but should in some manner be drawn away from it and securely anchored. To do this we have only to make an adaptation of Lexer's method as already described for inguinal hernia. The procedure will be as follows. Having isolated the sac and reduced its contents, ligate it as high up as possible, but do not yet remove it. Pass a long curved pair of forceps alongside the neck of the sac upward between Poupart's ligament and the peritoneum until the point of the forceps comes to lie against the fascia of the external oblique. Nick the fascia and deliver the point of the forceps. Now take a second pair of similar forceps and lock the jaws securely in those of the first. Withdraw the first pair, thus conducting the second pair down along the track of the first to the femoral ring. Now by means of the forceps so placed, seize the end of the sac and pull it forcibly upward and out through the little nick in the fascia of the external oblique. Anchor the neck here with one or two sutures and remove the redundant portion of the sac.

This bears some similarity to Kocher's technique. He proceeds thus: Having fully reduced the hernial contents, he seizes the tip of the sac in a pair of long curved forceps. He then inverts the sac until the tip of his forceps comes to lie against the anterior abdominal wall close against Poupart's ligament. He nicks the fascia and delivers the tip of his forceps carrying the sac. The sac is now drawn forcibly forward, ligated and removed. The neck, however, is not anchored in its new position but is allowed to slip back and may be expected to resume its old position at the internal orifice of the femoral canal. The purpose of Kocher is to get his ligature as high up on the neck of the sac as possible, but why, having once drawn the neck of the sac upward out of harm's way, he should immediately afterward surrender his advantage, it is somewhat difficult to understand.

Baldwin¹⁰ employs a very similar technique but purses the sac and leaves it *in situ*, getting his cue from Macewen. He claims that necrosis

of the sac does not follow; but for all that we believe that most surgeons, after a careful consideration of the points suggested, will agree that his success is due to his displacement of the sac-neck and that the leaving of the entire sac offers distinct disadvantages.

Enough has been said to make clear the principles for which we contend. The procedures suggested are free from all objections and offer great advantages over the methods more commonly in vogue. It is believed that, when surgeons in general understand more perfectly the object aimed at and always to be kept in mind in such operations as those of Macewen, Lexer and others, the old practice of leaving the neck of the sac to invite recurrence at the mouth of the hernial opening will become obsolete.

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THE EXTERNAL MALLEOLAR SIGN.

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WITH

A REPORT OF TEN CASES OF TRAUMA CAPITIS,

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Senior Interne, St. Louis City Hospital.

In order to make clearer the description of the external malleolar sign, concerning which I made a preliminary communication to the St. Louis Neurological Society, May 29th, 1911,* I present the details of a few cases with reproductions of photographs taken to illustrate the manner of inducing this sign and its manifestations.

"To determine its absence or presence the following procedures have been employed. With the patient in bed on the back, the lower limbs *relaxed* and extended, and wholly exposed, the soles and the ankles are readily accessible for the experiment. If possible, especially in doubtful cases, the limb to be tested should not be compressed or touched in any way except at the point chosen for the application of the stimulus. Recalling the possible reactions from indifferent skin areas, especially in profound cord lesions, will make the reason for this precaution clear. Of course, in restless patients it may be necessary to fix the limb, preferably by forcing the thigh against the mattress. With the patient lying on the side, the limbs may be bent at the knee; and by changing sides, each external malleolus is made accessible. Relaxation of the muscles of the legs and feet is very essential for the test in doubtful cases, as muscular relaxation is in tests for reflexes in general. When the patient can be examined sitting in a chair, the most convenient and satisfactory position and condition of the limb are obtained by having the limb to be examined resting on the examiner's knee (the latter sitting at a convenient distance) so that the limb is supported at the lower portion of the calf-muscles. The examiner can change from one side of the patient to the other, thus seeing perfectly, from the external aspect, the ankle, foot, and toes. This position permits the most accurate observation of the toe movements and perfect control of the point irritated and of the intensity of the stimulus applied.

**Interstate Medical Journal*, July, 1911.

"The irritation of the skin is best done with a rather dull steel point (a nail-file not sharp enough to wound the skin has served my purpose); in some cases the test can be made with a stout wooden toothpick, but absence of reaction to such irritation would not be conclusive. The area to be tested is the groove which outlines the external malleolus. In this groove, the point of the instrument used should be drawn from behind forward until the depression between the malleolus and cuboid is reached. This depression seems to be the most excitable point of the area. The degree of irritation applied should always be varied from slight stroking to rather severe scratching with considerable pressure, though it is never necessary to abrade the skin or cause actual pain. *Normally, this stimulus causes no movement whatever of the toes;* it does, however, in some cases, cause reflex contraction of the thigh-muscles exactly like that observed normally when the sole of the foot is similarly irritated. The abnormal reaction consists of extension or fanning of one or more,



Fig. 1.—J. D.

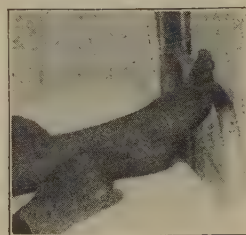


Fig. 2.—J. D.

or of all the toes; I am also convinced that a movement of *flexion* observed in a few cases, which were otherwise shown to have organic disease of the central nervous system, had the same pathological significance as extension had in other cases. In one case, in particular, with Babinski's sign on one side, there was flexion of all toes from the external malleolus of the same side; and before examination of the opposite side, prediction was made that the reversal of movement usually observed would occur on that side. It was found with normal plantar flexion to irritation of the sole. It is notable that this reversal of movement has been observed associated with the reversal of Gordon's sign in several instances. *A striking peculiarity of the external malleolar sign is the fact that with a unilateral Babinski, it is the rule to find the external malleolar sign on both sides.*

"The precautions necessary for this test are thorough relaxation of the limb; repeated and varied application of the irritation; care not to confound voluntary movements of recoil or escape with those excited reflexly. Often two toes, as the second and third, are extended together,

the others remaining unmoved—a movement almost, if not quite impossible of voluntary execution; fanning of the small toes, without extension of the great toe, is likewise practically impossible as a voluntary movement; abduction of the little toe, the others remaining unmoved, is also hardly possible by voluntary effort; but this movement alone has not seemed very significant. Of course, we need to take account of what practice might make possible in toe movements in exceptional instances; very few persons, however, have developed by exercise all the movements of which the toes are capable. Additional peculiarities which mark the abnormal movements excited from the external malleolus are the uniformity of the response with repeated application of the stimulus; their occurrence from a definite, limited skin area, most frequently from the depression mentioned (movements of escape occur from indifferent areas to painful or unpleasant irritation); and the electric-shock-like quality they frequently manifest. Attention given to these points will usually permit one to

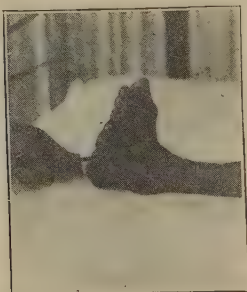


Fig. 3.—J. D.



Fig. 4.—J. D.

differentiate movements reflexly excited from the external malleolus, from voluntary or defensive jerks.

"An abnormal movement which I have previously described in association with the sign of Babinski, and which I have observed as remaining after that sign had disappeared, is slight inward turning of the foot with some dorsal flexion. This movement also occurs occasionally in association with the external malleolar sign. I believe it might have value as a pathological sign to a careful and skilled observer. At any rate, its associations are very significant."*

Case I.—J. D., aged twenty years. Fell from wagon striking face (right side) on pavement, August 23rd, 1911. Unconsciousness continued from time of fall until the morning of August 27th, when his mind cleared up so that he could talk, eat, and walk about. He had been brought to the hospital the evening of the 26th. August 27th, pulse 54, temperature normal. Complaining

*Quoted from my article: "An Explanation of the External Malleolar Sign Made with a View to Incite to the Study of It to Determine Its Place in Semiology," read before the Society of the City Hospital Alumni, June 15th, 1911; as yet unpublished.

of severe, unlocalized headache, almost delirious from pain. No paralysis; some abrasion (healing) on right side of face; both eyes black. Pupils normal. Deep reflexes all quite lively with slight indication of clonus at ankles. Plantar irritation gives normal flexion of toes on both sides—no indication of Babinski's phenomenon. Irritation of the inframalleolar skin area induces extension and fanning of all toes on each side.

August 28th.—Patient's condition somewhat aggravated. Walked some distance voluntarily to permit photographs to be taken. Fig. 1 shows right plantar flexion from irritation of the sole; Fig. 2 shows extension of all toes to irritation of the external malleolar area; Figs. 3 and 4 show like reaction on the left side.

August 30th.—Condition aggravated; temperature 101°. Babinski's phenomenon now present on right side; slight paresis of left side of face (Dr. Hagler's observation); other reflexes unchanged. Advised left subtemporal decompression because of presence of Babinski's sign on right; eventually, a like operation on the right side.



Fig. 5.—E. H.



Fig. 6.—E. H.

August 31st.—Operation (Drs. Rassieur and Bartlett): Left decompression revealed scarcely any increased tension; right decompression done immediately, great pressure found with subdural blood; drainage.

September 1st.—Patient had immediate relief; pulse 72. Babinski's sign has disappeared; right external malleolar sign still present; left plantar flexion, and no external malleolar sign found on left.

September 13th.—Patient has made uninterrupted progress towards recovery. The external malleolar sign is present on the right side; other reactions of toes normal. This abnormal sign may or may not disappear ultimately. Discharged, September 18th.

As an illustration of the ultimate result in a case of head injury, I give the history of Edward H.,* previously reported, and the examination of him made some months later.

Case II.—“Edward H., aged ten years. (Service of Dr. Blair.) Fall from tree; entirely conscious, never unconscious; no sign of skull trauma whatever. Slight weakness of entire left side. Complaint of headache. Reflexes (superficial) right side; nothing from external malleolus; normal flexion from sole; left side: extension of toes from sole and from external malleolus and from external

*Case II.—“A Preliminary Communication, etc.,” *Interstate Medical Journal*, July, 1911.

malleolus to ethyl chloride spray; but no reaction to ethyl chloride from left sole. Subtemporal decompression done on right side; great pressure found but no hemorrhage. Regained power immediately on right side, then lost it, then slowly regained it. Reflexes remained the same as before operation except that normal plantar flexion returned on left side leaving left external malleolar sign at the time of his discharge."

This history dates back to May, 1911. The patient again entered the hospital for typhoid fever late in July. His abnormal ankle sign was found by Dr. Lippmann who knew nothing of his previous history. Careful examination after recovery from typhoid gave the results shown in Figs. 5, 6, 7, and 8. Patient shows no paralysis; hiatus in right temporal region readily revealed to touch; not painful; he is bright and active. The figures show left Babinski with double external malleolar sign of the flexor type.

Case III.—A. M., adult male. (Service of Dr. Schwab and Dr. Leighton.) Fell from second story of barn, striking head and wrenching arms. Never any cere-



Fig. 7.—E. H.



Fig. 8.—E. H.

bral symptoms; paralysis of both arms, wasting of muscles, especially of shoulders, but no paralysis of lower extremities. Patient made slow progress toward recovery. At time of discharge was steadily regaining power in arms. That the injury to both brachial plexuses also involved the cervical cord was shown by the state of the reflexes in the lower extremities. Figs. 9 and 10 show the presence of the external malleolar sign on the left side with normal flexion from the sole; the opposite side presented a similar condition.

The three following cases are examples of central nervous lesions due to other causes than traumatism, in which the external malleolar sign was found in characteristic manner of manifestation.

Case IV.—P., male, aged thirty-eight years, admitted July 20th, 1911. Clear history of lues; eruption four months ago. Mentally disturbed, requiring restraint in bed; very severe headache; Argyll-Robertson pupils, left larger; double slight ankle clonus; external malleolar sign present on both sides, normal plantar flexion to irritation of soles. Figs. 11 and 12 show this fact as presented on the left side. The patient's condition improved under mercury, but he remained demented, and transfer to the City Sanitarium (Insane) became necessary in September. There was no change of the state of reflexes recorded. Patient died

September 17th in a cerebral seizure. Autopsy revealed softening of the left temporal lobe and of all the white substance of the right hemisphere; great congestion of entire brain. Softening was not due to closure of larger vessels; histological examination has not been completed.

Case V.—E. R., male, aged thirty-four years, admitted for second time in August, 1911. First admission July 5th, 1911, when he was in a confused, disorientated mental condition. Mind grew clearer and he was discharged after a few days, not having regained memory of short period preceding his admission. On second admission, brought in for mental observation. Slightly demented, indifferent; speech presents no peculiarities; no tremor. Lues twelve years ago. Argyll-Robertson pupils; lively, deep reflexes; external malleolar sign present on each side, with normal plantar flexion to irritation of soles, as shown in Figs. 13, 14, 15, and 16. Patient was transferred to the City Sanitarium with a provisional diagnosis of organic brain disease, probably dementia paralytica.

The external malleolar sign is so frequent, even in the early period of dementia paralytica, that it helped to clear up this case, in which the first attack was provisionally regarded as mental confusion due to heat and alcohol.*

Case VI.—Wm. J., colored, aged twenty-six. Denies lues; no external evidence found of it. Demented in a mild degree. Hemiplegic state (left side) dating



Fig. 9.—A. M.



Fig. 10.—A. M.

back eleven months. Presents characteristic signs of left hemiplegia, though face is not involved; pupils equal, but slow to light; platysma sign of Babinski; exaggerated deep reflexes in arm, with perfect wrist-clonus; deep and superficial reflexes (ankle, knee, cremasteric, abdominal) absent; Babinski's sign perfect, external malleolar sign on left side. Right side: all reflexes normal except that there is an external malleolar sign. These findings with regard to the plantar and malleolar reactions are shown in Figs. 17, 18, 19, and 20. Patient was transferred to the City Sanitarium on account of dementia. Examination made on September 18th, 1911, revealed the same findings.

This case is an illustration of the bilateral occurrence of the external malleolar sign in unilateral cerebral lesions. Elsewhere I have suggested that this phenomenon occurring thus bilaterally is due to lesion of crossed

*I now have records of 94 cases of dementia paralytica, males and females; Babinski's sign, with the ankle sign, was found eleven times; the ankle sign was found, single or double, eighty times; neither sign, fourteen times; unilateral Babinski and bilateral malleolar occurred seven times, showing the rule referred to above. Allowing for a certain percentage of error in diagnosis, still the percentage of cases of paresis in which the ankle sign is found is astonishingly large. [The author.]

and homilateral motor fibres by a single lesion. This explanation seems to be strengthened by the order of appearance and disappearance of this phenomenon and Babinski's sign: (1) Unilateral external malleolar; (2) Babinski's sign joining it, with (3) external malleolar sign on the other side; then (4) Babinski's sign joining the latter,—in traumatic cases. The reversal is: disappearance of Babinski's sign with the ankle sign first where they last appeared; then disappearance of the sign of Babinski where it first appeared, leaving the external malleolar sign first observed persistent for a time, perhaps to disappear or to remain as an indication of slight, though permanent, damage done to the central nervous reflex paths on one side.

The phenomenon of Babinski certainly occurs without the presence of the external malleolar sign, though rarely. I am convinced of this; and thus is shown, as by variations of association, the independence of these two signs. Study of their associations and dissociations may afford valuable diagnostic facts.*



Fig. 11.—P.



Fig. 12.—P.

The following record of ten cases of trauma capitis was made by Dr. Deppe of every traumatic skull case we saw together in the St. Louis City Hospital from August 6th to September 11th, 1911. The notes are made as brief as possible, only with a view to show the diagnostic value of variations of the reflexes and the importance to the surgeon of a good understanding of nervous semeiology.

Case VII.—Andrew C., aged five years. Shot in head by playmate. Admitted 9:15 a. m., August 6th, 1911. Unconscious; wound, apparently of entrance of bullet, at the outer margin of the right orbit; considerable swelling in right temporal region. Another wound which appeared to be made by exit of bullet at a point about midway between right parietal eminence and occipital protuberance. Pulse 132; pupils unequal, left larger, reacted sluggishly to light; all tendon reflexes abolished; skin reflexes absent on left side, diminished on right side; bilateral Babinski and external malleolar signs, more marked on the left side; projectile vomiting.

*I am much indebted to Drs. Burns, Deppe, Hagin, and Shroat, all on the staff of the St. Louis City Hospital, for their help in obtaining photographs and making clinical observations in the foregoing and numerous similar cases.

August 6th, 10:00 a. m. Unconscious; all skin reflexes abolished. Operation by Dr. Rassieur; both openings in the skull were freely enlarged; the cortex was found to be lacerated and there was considerable subdural hemorrhage; drainage.

12:00 m. Double external malleolar sign equal; Babinski not readily excited on right but easily on left; skin reflexes absent on left side, present on right; other reflexes as at first.

6:00 p. m. Moving hands, arms, and legs; talking; pulse 136. Babinski found on both sides, more marked on the left; external malleolar signs as before; other reflexes unaltered.

August 7th, 8:00 a. m. Pulse 120; mind clear; Babinski on both sides, less marked on right; external malleolar signs unchanged; tendon reflexes have returned, are active and equal; other reflexes unaltered.

August 8th, 8:00 a. m. Pulse 128; mental condition good; reflexes unaltered from yesterday.

August 9th, 8:00 a. m. Pulse 110; mental condition good; marked Babinski on left side, plantar flexion on right side, but at times Babinski can be obtained on right side; double external malleolar sign more marked on left side; other reflexes unaltered from yesterday.



Fig. 13.—R.

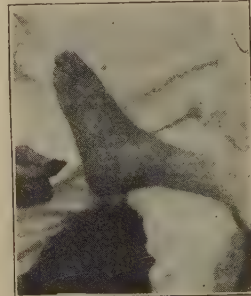


Fig. 14.—R.

August 10th, 8:00 a. m. Pulse 100; double normal plantar flexion (no Babinski); external malleolar on left side, absent on right; skin reflexes returning on left side.

August 22nd.—Improvement has been uninterrupted. Pulse 112; mind clear; no paralysis; pupils equal, and react well to light; tendon reflexes active and equal; skin reflexes active and equal; double normal plantar flexion, no external malleolar signs. Patient was discharged.

Case VIII.—Eugene C., aged twenty-six, laborer. Shot in head. Admitted August 13th, 9:00 p. m. Lacerated wound and hematoma over left frontal region about two inches above the left eye. Unconscious; pulse 64; right hemiplegia; pupils contracted and do not react to light; tendon reflexes present, exaggerated on right side; skin reflexes absent on right side, but active on left; Babinski on right side, normal plantar flexion on left side; no external malleolar sign, slight ankle clonus on right.

9:30 p. m. No response from right sole, fanning and extension of toes from irritation of left sole and at times a marked Babinski; external malleolar sign found on the left side, not on right. Incision over the hematoma revealed great intracranial hemorrhage. The effect of this was to cause disappearance of the left Babinski, but the external malleolar sign on that side remained.

10:00 p. m. Operation by Dr. Leighton. No anesthesia. The opening in skull was enlarged and there was considerable cortical hemorrhage; cortex was found pulpfied. A subtemporal decompression done on the left side revealed a tense non-pulsating dura, but no blood was found; wound packed for drainage.

August 14th, 2:00 a. m. Pulse 62; unconscious; no response from either sole; no external malleolar; tendon reflexes absent on right side, diminished on left; skin reflexes absent on left side, other reflexes as before.

8:00 a. m. Pulse 65; unconscious; moves right arm; plantar flexion on left side; no response from sole on right side; external malleolar sign on left side; skin reflexes present on left side; absent on right; tendon reflexes present on left side, absent on right; convulsive movements of left leg. Operation by Dr. Leighton. No anesthesia. Opening in frontal bone enlarged; bullet probed for but not found; drainage.

August 15th, 8:00 a. m. Pulse 64; unconscious; temperature 106.4°; plantar flexion from irritation of sole on left side, no response on right side; external malleolar sign on left; skin reflexes absent on both sides; death at 12:00 m.

Case IX.—William K., aged fifty-seven years, carpenter. Admitted at 11:30 p. m., August 18th. Unconscious; laceration over left eye; no fracture determined at this point; slight hemorrhage from right ear and from nose; pulse



Fig. 15.—R.



Fig. 16.—R.

110; pupils unequal, left larger, do not react to light; tendon and skin reflexes all absent; no response from either sole; no external malleolar sign.

August 19th, 8:00 a. m. Pulse 118; unconscious; double Babinski and double malleolar signs, more marked on right side; other reflexes as before.

10:30 a. m. Babinski diminished on left side, marked on right; external malleolar sign has disappeared on the left side and is elicited with difficulty on the right. Patient died at 11:00 a. m.

The autopsy revealed a fracture of the base of the skull extending from the left zygomatic process through both anterior fossae, the right middle and posterior fossae, ending about two inches above the foramen magnum on the right side; extensive subdural clot on the right side posteriorly, with smaller subdural clot on the left side.

Case X.—Edward N., age and occupation unknown, adult. Shot himself in the head. Admitted August 24th, 8:00 a. m. Unconscious; lacerated penetrating wound in the right frontal region with cortex protruding; hematoma over right parietal eminence. Pulse 120; pupils dilated, equal, irregular, reacted very sluggishly to light; no response from either sole; skin reflexes absent; tendon reflexes exaggerated, double ankle clonus.

8:45 a. m. Spastic condition of both legs; double ankle clonus; external malleolar sign on right, no response from either sole; pupils contracted and do not react to light; other reflexes as before.

10:00 a. m. External malleolar sign appearing on left side; other reflexes as before; clonic contractions occurring over the entire body; pulse 68. Operation by Dr. Young without anesthesia. Wound enlarged; cortex found to be badly lacerated; drainage.

11:30 a. m. No response from either sole; external malleolar sign absent on right side, diminished on left; other reflexes as before. Patient died at 12:00 m.

Case XI.—J. D. This case is Case I of this article. At the time of his discharge, September 18th, 1911, the following nervous findings were noted:—Pupils equal and reacting well to light; skin reflexes active and equal; tendon reflexes very lively, especially on the left side; double normal plantar flexion; pronounced external malleolar sign on the right side. Patient showed no paralysis; well.

Case XII.—George K., aged forty years. Hit on head by stray bullet that had evidently been fired in the air. Bullet had been extracted by a fellow workman. Admitted September 7th, 1911, 4:00 p. m. Conscious; laceration of scalp and



Fig. 17.—J.



Fig. 18.—J.

hematoma over the right parietal eminence. Incision revealed the fact that the bullet had penetrated the outer table of the right parietal bone and had caused depression of the inner table. Pulse 120; pupils unequal, right larger, both react well to light; tendon reflexes more active on right than left side; skin reflexes active on right, absent on left side; double normal plantar flexion; double Gordon, more pronounced on left side; double external malleolar sign, more pronounced on left side; some tendency to clonus at left ankle.

4:20 p. m. Babinski elicited at times on the left side; other reflexes as before noted. Operation by Dr. Reder. Ether anesthesia. Pulse 112; the opening in the parietal bone was enlarged, and the inner table was elevated; a small extradural clot was found at this point; drainage.

September 8th, 8:00 a. m. Conscious; pulse 99. Patient wants to get out of bed. No Babinski; other reflexes as before.

September 9th, 8:00 a. m. Pulse 88; wants to get out of bed. Gordon and external malleolar signs diminished on right side; other reflexes as before.

September 11th, 10:30 a. m. The patient's wife demanded his discharge. Pulse 84; pupils equal and react well to light; tendon reflexes active and equal; skin reflexes active on right side, diminished on left side; other reflexes unaltered. The patient was able to walk home.

Case XIII.—Sidney P., aged thirty-one years, laborer. Struck on head with

an iron bar. Admitted September 2nd, 9:00 a. m. Conscious; incised wound about three inches long over the left temporo-parietal area with a linear fracture of the left parietal bone; no depression. Pulse 80; pupils unequal, left larger, both reacting well to light; tendon reflexes active and equal; skin reflexes all absent except the left abdominals, which are weak; normal plantar flexion on the left side, no response from the right sole; external malleolar sign on the left side, but not on the right.

11:00 a. m. Pulse 86; Babinski elicited at times on the right side; normal plantar flexion on left; double external malleolar sign more marked on the right side; other reflexes as before.

September 3rd, 8:00 a. m. Pulse 88; conscious; double normal plantar flexion; double external malleolar sign; skin reflexes active on left side, diminished somewhat on right side; other reflexes as before.

September 7th, 8:00 a. m. Improvement has been uninterrupted; pulse 84; pupils equal and react well to light; tendon reflexes active and equal; double normal plantar flexion; the malleolar sign has disappeared on both sides. Patient discharged.

Case XIV.—Christ W., aged thirty years, laborer. Fell down elevator shaft fifteen feet. Admitted September 9th, 2:15 p. m. Somewhat cloudy mentally; small laceration and hematoma found over occipital prominence in middle line.



Fig. 19.—J.



Fig. 20.—J.

Pulse 78; tendon reflexes active and equal; skin reflexes active and equal; double normal plantar flexion, but Babinski can be excited from outer border of sole on the right side; no external malleolar sign on either side.

3:15 p. m. Pulse 50 and irregular; stupid; Babinski less readily excited on the right; external malleolar appears on left side; other reflexes unaltered. Lumbar puncture reveals bloody cerebrospinal fluid under abnormal pressure. Slight epistaxis with vomiting of blood.

7:30 p. m. Patient stuporous; double plantar flexion; double Gordon, more pronounced on right side; marked external malleolar on the left side.

September 6th.—Still stuporous, complaining of very severe headache; pulse 56; changes of reflexes consist of double plantar flexion, double Gordon, more pronounced on right, and double external malleolar, more marked on right.

September 7th, 8:30 a. m. No mental change, still great headache; tendon reflexes livelier on right side; skin reflexes diminished on right side; double plantar flexion; double Gordon, more marked on right side, and double external malleolar, more marked on right. Operation, ether anesthesia, at 10:00 a. m. (Dr. Rassieur for Dr. Bartlett.) Pulse 56; left subtemporal decompression revealed fracture of sphenoid and extradural basal clot with intradural pressure and some petechial hemorrhages into brain substance exposed; drainage.

1:00 p. m. Conscious; headache; reflexes unchanged; pulse 80.

September 8th, 8:00 a. m. Conscious, but sleepy; pulse 60; skin reflexes now equal; other reflexes and signs as previously noted.

10:00 a. m. Operation; right subtemporal decompression; contusion of cortex found.

5:00 p. m. Conscious; headache; pulse 58; no alteration of reflexes found from conditions previously recorded.

September 23rd.—Patient is recovering, but has been "flighty" mentally. Headache ceased on September 10th. Reflex signs remaining: right plantar flexion, at times plantar fanning; from right external malleolus, at times abduction of little toe; left plantar flexion; abduction of little toe on left from irritation of left external malleolus; pulse 72.

Case XV.—Charles B., aged twenty-five, chauffeur. Injured in an automobile accident. Admitted September 9th, at 6:20 a. m. Conscious, but it is impossible to get any history from him. Hematoma over the right temporal region; free hemorrhage from nose and mouth; ecchymosis and great swelling of lids of both eyes; pulse 92; pupils equal, react well to light; tendon reflexes active and equal; skin reflexes active and equal; double normal plantar flexion; no Gordon or external malleolar sign on either side.

3:30 p. m. Patient is stupid; pulse 64. Owing to ecchymosis and swelling of lids pupils cannot be examined; tendon reflexes very active and equal; skin reflexes absent on left, active on right side; double Babinski excitable at times; double Gordon indicated; double external malleolar sign; patient states that he is blind in the right eye.

6:00 p. m. Patient's general condition has not altered. Operation under ether anesthesia by Dr. Young. Right subtemporal decompression revealed fracture of the temporal bone with depression and an extradural clot of considerable size with some pulpification of the cortex and subdural hemorrhage.

September 10th, 8:00 a. m. Still stupid; skin reflexes absent on left, diminished on right side; Babinski on left side, normal plantar flexion on right side; no Gordon on either side; external malleolar sign on both sides; other reflexes as before.

September 11th, 8:00 a. m. A little stupid; pulse 68; double plantar flexion; no Gordon; double external malleolar sign; other reflexes the same.

September 12th.—Mentally clearer; pulse 80; double normal plantar flexion; no Gordon; double external malleolar; skin reflexes active on right, diminished on left side.

September 13th, 8:00 a. m. Brighter mentally; pulse 68; external malleolar sign present on the left, absent on the right side; other reflexes the same.

September 16th.—Improvement has been uninterrupted; mind clear; pulse 72; no paralysis; vision in right eye has improved; tendon reflexes active and equal; skin reflexes active and equal; double normal plantar flexion; no Gordon; external malleolar at times on the left side, absent on right; pupils equal and react well to light. Patient was discharged convalescent.

Case XVI.—Willie S., aged five years, newsboy. Fell from street car. Admitted September 11th, 6:55 p. m. Conscious; large hematoma over the left parieto-occipital area; bleeding from left ear. Pulse 100; pupils equal and react well to light; tendon reflexes active and equal; skin reflexes active on left side, diminished on right; double Babinski, double external malleolar sign; both more marked on right side; no Gordon.

September 12th, 10:00 a. m. Patient stupid; double Babinski, Gordon, and external malleolar signs, all more pronounced on the right side; other reflexes as noted.

1:30 p. m. Operation under ether anesthesia by Dr. Rassieur. Pulse 90.

A stellate fracture of the left parietal bone with depression and extradural clot was found. A considerable area of the parietal bone was removed.

September 13th.—Conscious; pulse 92; Babinski on both sides, but more marked on the right; no Gordon; double external malleolar more marked on right side; other reflexes as noted.

September 14th, 8:00 a. m. Pulse 90; reflexes as noted.

September 15th, 8:00 a. m. Conscious, playing in bed; pulse 88; skin reflexes active on left, somewhat diminished on right side; double plantar flexion, but Babinski can be brought out at times on the right side; other reflexes as noted.

September 17th.—Patient is bright; pulse 90; no paralysis; no pain, pupils equal and react well to light; tendon reflexes active and equal; skin reflexes active and equal; double plantar flexion, no Babinski, no Gordon; external malleolar sign brought out at times on the right side, absent on the left side.

3705 Delmar Boulevard.

MEDICAL AND SURGICAL PROGRESS.

ADVANCES IN CANCER RESEARCH.

A REVIEW OF RECENT LITERATURE.

By MOYER S. FLEISHER, M. D., of St. Louis.

1. White and Loeb: The Influence of Harmful Physical Agents (Warm) on the Growth of Tumor Cells. (*Centralbl. fuer Bakt. Parasit. u. Infek.*, Vol. 56, p. 325, 1910.)
Transplantation of Stationary and Retrogressing Tumors. (*Centralbl. fuer Bakt. Parasit. u. Infek.*, Vol. 56, p. 488, 1910.)
The Influence of an Inoculation with Tumor Material of Experimentally Decreased Virulence Upon the Result of a Second Inoculation with Tumor Materials of Experimentally Decreased Virulence. (*Proc. Soc. Exper. Biol. and Med.*, Vol. 8, p. 22, 1910.)
2. Lambert: The Influence of Mouse-Rat Parabiosis on the Growth in Rats of Transplantable Mouse Sarcoma. (*Journ. Exper. Med.*, Vol. XIII, No. 2, 1911.)
3. Rous: A Transmissible Avian Neoplasm. (*Journ. Exper. Med.*, Vol. XII, No. 5, 1910.)
A Sarcoma of the Fowl Transmissible by an Agent Separable from the Tumor Cells. (*Journ. Exper. Med.*, Vol. XIII, No. 4, 1911.)
The Relations of Embryonic Tissue and Tumor in Mixed Grafts. (*Journ. Exper. Med.*, Vol. XIII, No. 2, 1911.)
4. Buschke: The Inoculation of Rat Sarcoma and Mouse Sarcoma Into Newly-Born Animals. (*Berlin. klin. Wochenschr.*, Vol. 48, p. 215, 1911.)
5. Loeb: The Parthenogenetic Development of Ova in the Mammalian Ovary and the Origin of Ovarian Teratoma and Chorion-epithelioma. (*Journ. Amer. Med. Assoc.*, Vol. LVI, p. 1327, 1911.)
Rapid Simultaneous Appearance of an Adenocarcinoma and Squamous Cell Carcinoma at the Nipple of a Mouse. (Reported at a Meeting of the American Association for Cancer Research, at Buffalo, April 12th, 1911.)
6. Gruenbaum and Gruenbaum: The Results of Some Experiments Undertaken to Test the Hormone Theory of the Causation of New Growths. (*Journ. Path. and Bact.*, Vol. XV, p. 289, 1911.)
7. Rohdenburg, Bullock and Johnston: The Effects of Certain Secretions Upon Malignant Tissue. (*Archives of Internal Medicine*, Vol. 7, p. 491, 1911.)

While research in cancer by means of experimentation upon lower animals has not as yet been successful in bringing to light any evidence regarding the cause, nor any definite knowledge which might point to a possible means for curing this disease, still, many interesting facts have been learned concerning the growth of tumors and concerning the possibilities of producing artificial immunity in these lower animals. And these observations certainly are to be considered as the early stepping-stones leading toward a fuller knowledge of cancer.

The accumulated results of experimental research have made more and more evident the fact that the development of our knowledge of the growth of normal tissue, and the factors and stimuli influencing such growth, will be of great importance in shedding light upon our knowledge of cancer. We find that cancer cells differ less in their reactions of growth from normal cells than was formerly considered to be the case. With the view of extending and correlating knowledge of the growth of normal and cancerous tissue, we find that to-day many investigators are making a careful and close analysis of the growth of both these tissues.

White and Loeb have continued and amplified some earlier experiments of the latter's, concerning the influence upon the growth of inoculated tumors. Loeb had found that heating a tumor to 44° C., for some time before inoculation, retarded the rate of growth of the graft, lengthened the period of incubation—namely, the period elapsing between the inoculation and the earliest appearance of a palpable mass at the site of inoculation, and that as the exposure of the tumor to the heat was lengthened, the number of tumors which retrogressed increased. In this recent work the influence of heating upon the growth of the tumor has been studied in detail and the relative influence of heating for various lengths of time has been analyzed. Thus it was noted that within certain limits, that is, up to about forty or forty-five minutes, the period of heating has no influence upon the number of "takes": successful inoculations in which the tumor gave evidence of growth, independently of whether it continued to grow or only grew for a short time and later retrogressed. When the period of heating was continued over an unusually long time, the number of "takes" did fall below the average; thus the number of "takes" gives evidence only of the more marked harmful influence.

The latent period, growth, energy, and numbers of retrogressed tumors show a common relationship to the time of exposure to heating, and, to a certain degree, the influence upon these qualities is proportional to the length of time of heating. The latent period becomes gradually longer, the rate of growth of tumor more sluggish, and the retrogressions more common as the exposure to the heating increases. Therefore, it appears that these three activities are more delicate determinants of harmful influences than is the number of "takes," and it is probable that these three evidences of the tumor growth are dependent upon one and the same properties of the cell protoplasm.

It was further noted that the tumor cells recover from the harmful influence, provided this does not exceed a certain limit. The inheritance of the retarded rate of growth resulting from the heating lasts during only a few generations, for the heated tumors may eventually grow as large or even larger than unheated tumors. The recovery of the tumor cells from the influence of the heating may be aided by reinoculation into another animal. Even in tumors, which have been heated before

each reinoculation, and in which this heating has been repeated during several generations, no summation of the external harmful influence is noted, no immunity to the same, nor yet a selection of resistant cells. Thus one notes the evident recuperative power and great elasticity of tumor cells.

In view of the apparent resistance of tumor cells to harmful influences and their power of recuperation from the same, White and Loeb believe to have brought forward fresh evidence that tumor cells are immortal, and that as tumor cells usually arise from somatic cells, these latter cells, like germinal cells, must be considered to have at least potential immortality.

In a second communication, White and Loeb have studied the growth-energy of tumors which were transplanted from stationary or retrogressing tumors, using in these experiments rat sarcoma, lymphosarcoma of the dog, and carcinoma of mouse. They found that in animals inoculated with such tumors the growth-energy of the graft was less than in animals inoculated from an actively growing tumor.

Differences were noted in the various kinds of tumor; thus mouse carcinoma gave relatively better results upon reinoculation from a stationary or retrogressing tumor than did the lymphosarcoma of the dog.

Further, the stage of retrogression at which the transplantation was undertaken influenced the result, better results being obtained when the tumor was transplanted during the earlier stages of retrogression.

By repeated transplantations it was possible to restore a retrogressing tumor to its original "virulence"—namely, to bring it once more to the rate of growth of the original strain. Here again is evidence of the elasticity and recuperative powers of tumor cells.

The means, which have been used in these experiments to restore to the tumor its original properties, have been purely mechanical—to wit, the handling during transplantation, and are comparable to the stimuli which normally give rise to regenerative processes. The removal of the pressure of the connective-tissue capsule must be considered as a factor in the restoration of the energy of the tumor cells.

In addition, by the transplantation the tumor cells are placed in a new environment and may thus be relieved from certain unfavorable influences which may have existed in the earlier host.

In his earlier work, Loeb had thought of the possibility of using the tumor material of experimentally-decreased virulence as a protective vaccine against a second inoculation with more virulent tumor material; and White and Loeb have now reported on some preliminary experiments in this connection.

They found that if the heated tumor material did not give rise to any growth, no immunity was noticeable. If the preparatory inoculation (with tumor material of experimentally-decreased virulence) gave rise to a temporary growth which was followed by spontaneous retrogression, an immunity against subsequent inoculation was obtained in many, but not in all cases. When mice are inoculated with heated tumor material, and this grows, they can be successfully inoculated a second time with tumor material of experimentally-decreased virulence. In some cases both tumors grow, and the second may grow as well as in mice in which the first inoculation did not take. Usually, however, only one tumor grows after two successive inoculations with material of decreased virulence, and sometimes it was the first inoculation that grew, or again, only the second inoculation grew.

Thus it appears that in a certain number of cases the growth of a tumor protects, to some extent, against a subsequent inoculation with tumor material of experimentally-decreased virulence. This protective influence does not seem to be present, however, in all cases. The variable energy of growth of the first and second tumors probably complicates the results, and these studies must be considered as only a preliminary report.

It is well known that when a mouse tumor is transplanted into a rat or vice versa, that the implanted tumor will show signs of growth for eight to ten days, but that later the tumor is destroyed and absorbed. Ehrlich explains this primary growth as being due to the carrying over with the tumor material of a specific food substance, *X-Stoff*, which is, however, absent in the new host. Since the *X-Stoff* brought over with the mouse tumor is soon exhausted, the tumor material cannot live in the new host. Lambert has attempted artificially to improve the conditions for the growth of mouse tumors in rats by operatively joining rats and mice so that there is an interchange of the body fluids.

By means of the mouse-rat parabiosis, Lambert hoped to supply to the strange host (in this case the rat, since he used a mouse sarcoma) some of the nutritive fluids of the normal host, and thus to increase the period of life of the transplanted tumor. He found that under the influence of the mouse-rat parabiosis, the pieces of mouse tumor transplanted into rats grew for at least seventeen days, thus considerably longer than when no interchange of body fluids took place. He was unable to continue the parabiotic condition for longer than eight to ten days, as the mice usually died at this time. He states that his results might be accepted as confirming Ehrlich's hypothesis; but, in view of the complexity of the factors involved, he recognizes the possibility of other explanations of his results.

The methods used to discover a parasite which might be considered as the causative agent in cancer, or to obtain some evidence pointing to a parasitic origin of cancer, have been multitudinous. Rarely has any definite evidence been obtained which points to such an origin. Rous has, however, been able to obtain evidence which points to the parasitic origin at least of the tumor which he used in his experiments.

Rous has been able to transplant a sarcoma noted in a hen into other hens—this being the first transplantable avian tumor reported. At first it was possible only to transplant this tumor into fowls very closely related to the original hen in which the tumor arose. Later, after the tumor had been transplanted through several generations, it was found possible to transplant it into other less closely related fowls. That this tumor was a true sarcoma there was very little doubt, since the grafts developed from the transplanted cells. Metastases take place usually by the bloodstream, and animals in which the tumor has retrogressed are immune to a second inoculation. Thus this avian tumor presents the main features presented by the ordinary transplantable tumors of mice and rats. Bacteriological examination of this tumor proved negative. It was, however, found that the injection of the cell-free filtrate of the crushed tumor material (obtained by passing through a Berkefeld filter) into hens led to the formation of a tumor which was in every respect similar to the original tumor. It was also possible to produce this tumor by the injection of the dried and powdered tumor material which had been kept at room-temperature for many days.

As a possible explanation of his results, Rous suggests two possi-

bilities: First, there may be a minute parasitic organism which acts as the self-perpetuating agent of this tumor; secondly, the cells of this tumor may elaborate a chemical substance which acts upon the cells of the injected animal in such a manner as to cause these cells to take on the qualities of the sarcoma. These latter cells would again produce this chemical stimulant, and thus perpetuate the sarcoma. Rous does not, however, adopt either one of these theories as proved, but leaves the true explanation to be determined by further investigation.

The theory that tumors arise from misplaced embryonic tissues has long had many supporters, and many attempts have been made to obtain proof to uphold this theory by experimental means. Rous has lately studied the relations existing between embryonic tissue and tumor tissue when these are injected in a mixture into susceptible animals. From the point of view of obtaining support for the theory of the embryonic origin of tumors, his experiments, like those of his predecessors, have failed; the embryonic tissue grew no more actively than when injected alone, and eventually was absorbed as it is in animals when injected without the admixture of tumor material.

Rous further found in these experiments that occasionally a direct union existed between the tumor tissue and some of the stratified epithelial tissue of the embryo, and that the two kinds of cells were so intermingled that it was impossible to delimit the one from the other. Rous argues, as a result of these observations, that one is not justified in all cases where there is an apparent continuity between normal and carcinomatous tissues (especially in epithelioma) in accepting such histological evidence as proof of the origin of a cancer from normal tissue.

A recent observation of Loeb's is of interest: Loeb discovered in a mouse which had an adenocarcinoma of the mammary gland, a second tumor—namely, an epithelioma. The mammary carcinoma had grown upward toward the skin, and at certain places where the mammary tumor had come in contact with the skin an epithelioma had developed, and was growing down into the underlying tissues. It seemed probable that the adenocarcinoma (which is the most common type of tumor among mice) had been primary, and that the epithelioma (which is relatively rare among mice) was secondary. Whether the origin of the second tumor was due to a specific organism being borne to it by the cells of the mammary tumor, or whether this epithelioma developed as a result of the irritant action of the carcinoma cells, cannot be determined, but it seems probable that the epithelioma arose as the result of some action of the first tumor.

Another interesting observation of Loeb's throws some light upon the development of teratoma. In studying the ovaries of guinea-pigs at various stages of the sexual cycle, Loeb has noted structures which appear to be parthenogenetically developing ova. In some cases, chorionic vesicles and syncytial cells were noted and a structure which is probably to be interpreted as a neural tube. Such developing ova were seen by Loeb in about 10 per cent. of young guinea-pigs' ovaries; and since the full history of the animals used was known, it is possible to exclude the suggestion that the ova were fertilized *in situ*. It seems probable that altered conditions in the ovaries (variations in blood-pressure or intra-follicular pressure, and a change in the oxygen supply) may account for the parthenogenetic development of these ova.

In view of these findings it would appear that teratoma and chorio-epithelioma arise from parthenogenetically developing ova, and not from misplaced blastomeres.

Although cancer among human beings (and among animals also) is considered to be rather a disease of old age, it has been stated by Bashford and his co-workers, that in inoculating animals with pieces of tumor material, the younger animals gave better results, that is to say, a larger number of successful "takes" than did the older animals. Buschke has carried these experiments still further, and has attempted to determine whether new-born animals were susceptible to the inoculations of tumors; for this purpose he has used both rats and mice. While he made no observations regarding the relative growth in new-born animals as compared with the growth in older animals, his results show definitely that the very tender age of the animals apparently exerted no influence upon the success of the "takes."

Gruenbaum and Gruenbaum have attempted to determine the influence of the internal secretions of certain glands upon the growth of tumors in rats. They inoculated into animals, which had developed immunity to cancer, a mixture of rat tumor and parotid gland. While none of the rats developed tumors, they found that the simultaneous inoculation of parotid gland seemed to stimulate the growth of the tumor, so that at least for a short time the tumor grew in the immune animals. If parotid gland was inoculated a second or third time into such animals, the growth of the tumor seems again to be stimulated. They further tested the influence of pituitary body, suprarenal bodies, kidney, testes, and liver, and found these organs to have a slight effect upon stimulating growth, but not as marked an effect as the parotid gland.

When the parotid gland was removed from animals, it appeared in a few cases that the growth of the inoculated tumor was interfered with; fibrosis of the tumor was noted in these few cases.

These observers consider their experiments as offering some slight evidence in favor of the hormone theory of the causation of new growths, and suggest that in the development of a tumor certain hormones, which serve to stimulate growth, may act in two different manners. First, there may be an actual and marked hypersecretion of some substance which stimulates growth; secondly, there may be a diminished absorption of some hormone in one part of the body, thus causing a hypernutrition at another part, as a result of which latter there is a hyperstimulation of growth.

Rohdenburg, Bullock and Johnston have also studied the influence of internal secretions upon tumor growth. They have studied especially the influence of the secretions of ovaries, testes, thymus, and thyroid upon the growth of tumors and immunity to tumors.

They found that removal of these various glands seemed to increase the number of spontaneous retrogressions of the grafted tumors.

The removal of these organs, especially the removal of both the thymus and the testes, prevented the development of immunity. Extracts of many organs all of which were treated in a similar manner,—the technique of obtaining these organ extracts is not described in this article,—when injected into animals with small tumors, caused a retrogression of these tumors; occasionally larger tumors retrogressed when such treatment was administered. They believe that possibly as a result of the special methods used for obtaining the organ extracts, the same specific chemical compound is obtained from all organs. By the injection of such gland extracts, mice were immunized against a later inoculation with tumor material. An action of these gland extracts upon tumor cells was sought for *in vitro*, but no evidence of any action was noted here.

The experimental portions of this article seem to be based upon a relatively small number of experiments, and in some cases the results are not clear, and the differences between the control animals and the animals treated by various experimental means are not very sharp.

Rohdenburg, Bullock and Johnston further report here concerning a large number of cases of cancer in human beings treated with organ extracts. Their best results were obtained with extract of thymus gland, and in cases so treated they noted diminution of pain, general improvement, and some diminution of the glandular involvement. In one case—a small recurrence of a mammary carcinoma—they report a cure.

RELATION BETWEEN FUNCTIONAL AND ORGANIC PROCESSES.

A REVIEW OF RECENT LITERATURE.

By C. FISCH, M. D., of the Editorial Staff.

1. Fischer: The Edema. Translation into German by Carl Schorr and Wolfgang Ostwald. Berlin, Theodor Steinkopf, 1910.
2. Fränkel: Dynamic Biochemical Study of Vital Processes. Wiesbaden, 1911.
3. Von Hansemann: Descendency and Pathology—Comparative Studies and Thoughts. Berlin, August Hirschwald, 1910.
4. Klose and Vogt: Clinic and Biology of the Thymus Gland. Tübingen, H. Laup, 1910.

This short discussion of the interrelation of two processes of different types has been suggested by a book published some time ago, of which the author is von Hansemann, of Berlin, a well-known and able worker in pathology, and widely familiar with all the other branches of medicine and natural science, as well as the study of the different and identical characters in the life of animals and vegetables. In addition to the results of his own experiments, the contents of the book comprise a critical discussion of the enormous amount of work hitherto undertaken in the hope of explaining descendency; hence these excellent qualities must be a source of information for all who wish to know the different theories, or what books or journals should be consulted if further knowledge is required. Undoubtedly we have here the result of labor covering many years.

This book, then, will be the main portion of my review. Its chapters—introduction, preformation, species and varieties, variability, condition of constancy, altruism, purpose (*Zweckmässigkeit*), orthogenesis, Lamarckism, functional adaptations, epidemics, physiological death, and conclusions—show conclusively its breadth and what advantages must accrue to the reader who follows the lines in a scientific spirit. But it is impossible to go into each chapter in detail, for the space required would be all out of proportion with what is allotted for this review in the JOURNAL. Nevertheless attention should be called at once to the second chapter, which is really the basis of all the others. It deals with the explanation of the wonderful capacity of the ovum to produce the diametrically opposite structures and functions of the grown organism. The name "Idioplasm" has been used for a substance that is present in every cell arising from the ovum. This, of course, would mean that every cell must be the same as the other, and that idioplasm in itself cannot cause the differentiation between the different organs, tissues and cells. For this reason, other theories were worked out to prove that idioplasm was not a substance *per se*, but that the ovum contained a great number of minute particles of different functional character that were transmitted to the division cells. This led to the belief that these different particles could be found in various quantities in single cells. But granting this, how is it possible that they are always transmitted in the same quantity?

In reply to this question, no scientific proof has been advanced; it is a biological predestination. The formation of the adult organism, being uniformly regular, proves without a doubt, that this theory can be easily attacked.

In plants idioplasm is thought to be distributed through the whole plant. It is possible by cutting a small piece of a begonia, for instance, to produce a new plant. In the animal body, however, idioplasm is more or less confined to the organs of reproduction. Experience shows that in higher animals the loss of any member of the body is not replaced by new growth; this obtains in mammals and birds. On the other hand, in the lower animals we find the organs reforming; for example, a tail, a leg, and even a head. This shows that idioplasm must be present, even in the adult state, in every cell. In still lower animals, in the lumbricus, for instance, the worm can be cut into pieces and each piece will grow out into a complete worm.

Von Hansemann, though not opposed to the discussion of various theories, is firmly convinced that there exist in an ovum specific substances. He calls this a mechanical explanation in contradistinction to the biological idea of a sensitive origin of the differentiation of the cells; but though his contention would at first appear against a biological interpretation, it is nevertheless of the same nature. Biology is an individual, not a natural science. The author's discussion of the cause of tumors is, of course, on the usual lines; and he points out that so far we do not know anything positive about the causation, and that although all animal transmissions are interesting, no further light has been thrown on the nature of cancer, despite Bashford's assertion to the contrary. It can readily be seen that von Hansemann's ideas are in close relation with idioplasm and his conclusions are that the early separation of cells from their normal location may cause this special modification; he denies, however, that embryonic cells can remain alive for a long time in the growing organism. That the first division-cells of the ovum have the same quality as the ovum itself is proved by the teratomata and by the chorion-epitheliomata in males. Of course, it is mechanically impossible to advance proofs; in a direct manner, that would hold, but we know this much, that other facts in pathology and other sciences rest upon similar bases. The specific agent, the cause of a natural process, has no significance, since it is a mere term, and furthermore the beginning and ending of all natural processes are non-existent. We must, however, regard a cause as a premise in our scientific work, just as belief in a Higher Power is the keystone of religion. The word "absolute" should be eliminated from all scientific discussion.

What has been said in this paper in regard to the second chapter of von Hansemann's book applies to all the other chapters. But though all of them are above the ordinary, the discussion of epidemics, and especially of physiological death, is so interesting that a translation of the latter would not be out of place here.

"I have mentioned," says the author, "all those facts which are generally responsible for the disappearance of races. The question is, however, whether the cause is physiological or pathological. Certainly the destruction of the species by catastrophies cannot be characterized as physiological. Where the extermination is the result of the disastrous effects of climate or of epidemics brought into communities by a strange species, the cause must be held as pathological. The destruction which results from the evils of the products of digestion is, in my opinion, also patholog-

ical, while on the other hand, the destruction which is caused by the natural wear and tear of life on account of our "altruism" is physiological. There is, however, a great difference between the death of a species and that of a single individual. This difference is due to the fact that a species can avoid death by changing its place of habitation or by adapting itself to a new way of life. But where specialization characterizes the manner of life of a species, a decreased capacity for variation, whether change of habitation or a new mode of life, follows, and hence the species is decimated. In these circumstances the death of a species, similar to the physiological death of the individual, is as natural an outcome for the species as for the individual."

On the whole von Hansemann believes that there is a descendency; this, of course, is only a personal belief, since he is unable to give any better proof than circumstantial evidence. He accepts Darwin's theory of natural selection, although he is aware that there are no facts to substantiate this theory. Hence he is a biologist who follows in the footsteps of Darwin. All this, however, does not detract from the great value of a book that stimulates the reader's thoughts in regard to problems which as yet have not been solved.

One important defect in von Hansemann's discussion is his consideration of cells as builders of the body,—as individual sources of the normal functional state of the organism. He forgets that the cells are only the division of the whole and are differentiated in specific character by physical and chemical (mechanical) reactions which are in process. At no distant time, the subject of the dependence of normal equilibrium of the whole organism will be satisfactorily studied, and then we shall ascertain that the minute portions—the cells—are not the sources of the normal functional state of the organism. The interrelation between the various parts of the body is sufficient evidence that the whole organism is the activating factor in what is known as the normal state. Our knowledge of the cells has taught us much, but we must not forget that cells do not control the organism, but that the organism controls them. This interrelation is called by him altruism.

The difficulty attending exact physical and chemical study, so as to arrive at definite data in regard to the changes in an organism, is shown by the labors of Klose and Vogt when they made a thorough histological investigation of the thymus and of the effect of thymus resection. What they ascertained has shed a great deal of light on the question whether the thymus should be removed or not, a matter which was first agitated by Basch. To arrive at any definite conclusions, some knowledge as to the true function of the thymus was absolutely necessary. As is known, the thymus has a neutralizing effect by rendering nuclein, or its combination with phosphoric acid, harmless. When nuclein is removed, intoxication follows. This hypothesis rests on the fact that when nuclein is absent from the organism, there are loss of calcium in the bone, lessened alkalinity or acidity of the spinal fluid, edema of the cerebral tissues and of other structures. If we admit this, the conclusions arrived at by Klose and Vogt are wrong. Embden, for instance, has shown that when nuclein is absent nothing out of the normal can be found. This shows how uncertain so-called exact studies are in regard to the interdependence of cause and effect in metabolism.

Fischer has done some excellent work so as to find out the obscure origin of the edema which follows the removal of nuclein. He attempted to solve the problem by working on the colloid-chemical basis, and his

experiments were limited to the muscles of frogs. The cause of edema, according to this experimenter, is in the tissue itself. Further experiments were undertaken to explain the swelling of fibrin, gelatine, and other substances. But in Fischer's work there is an absence of facts of a definite nature. To illustrate, he insists that the secretion of the kidneys is due to a relationship between different colloid-chemical reactions. This sort of conclusion, as well as the others, is biological, and must be criticized on the ground of being illogical, since it is based on preconceptions; a fact that destroys the objectiveness that should be inseparable from all scientific undertakings. Clinically considered, Fischer's explanation of edema is of no help, and even here, though his explanation is an honest attempt, he fails to convince.

The second part of Fränkel's "Dynamic Biochemical Study of Vital Processes" is also proof of the fact how imperfect are the explanations of the natural processes. The term "biochemistry" is a *contradictio in adjecto*, since life and chemistry are terms that bear no relation to each other. Fränkel deals with the physical and chemical processes in the living organism and gives a clear explanation of single chemical changes. This is not the place to go into detail with a view of giving a complete account of the various phases of the problem. The value of Fränkel's book does not lie in correlated facts, but in the belief that he has established a purposeful (*Zweckmässig*) series of single facts. But the greatest claim that his book has on the attention of the reader is that it shows that our present ideas, as to the relation between functional and organic processes, contain so much that is hypothetical that it is impossible to interpret a natural process satisfactorily by pursuing only one line of reasoning.

SALVARSAN IN OPHTHALMOLOGY.

A REVIEW OF RECENT LITERATURE.

By JOHN GREEN, JR., M. D., of the Editorial Staff.

1. Best: Salvarsan in Ophthalmology. (*Muench. med. Wochenschr.*, No. 48, 1911.)
2. Cohn: Salvarsan and Eye. (*Wochenschr. fuer Ther. u. Hyg. des Auges*, October 6th, 1910.)
3. Coover: Salvarsan and Syphilis of the Eye. (*Journ. Ophthalmology and Oto-Laryngology*, June, 1911.)
4. de Lapersonne and Levi: Salvarsan and Diseases of the Eye. (*Archives d'Ophthalmologie*, January, 1911.)
5. de Schweinitz and Shumway: A Case of Syphilitic Iritis Treated with Salvarsan. (Section Ophthalmology, College of Physicians, October 20th, 1910.)
6. Denig: Two Cases of Interstitial Keratitis and One of Optic Neuritis Treated by Salvarsan. (*Ophthalmic Record*, December, 1910.)
7. Flemming: Action of Salvarsan on the Eye. (*Archiv. fuer Augenheilkunde*, Bd. LXVIII, 1911.)
8. Michaelis: Salvarsan in Diseases of Eye. (*Deutsch. med. Wochenschr.*, No. 49, 1910.)
9. Nacht: Salvarsan in Syphilis of the Eye. (*Wochenschr. fuer Ther. u. Hyg. des Auges*, December 1st, 1910.)
10. Schnaudigel: Salvarsan in Ophthalmology. (*Archiv. fuer Augenheilkunde*, Bd. LXVIII, 1911.)
11. Seeligsohn: Salvarsan in Interstitial Keratitis. (*Berl. klin. Wochenschr.*, No. 41, 1911.)
12. Stuelp: Salvarsan in Ophthalmology. (*Wochenschr. fuer Ther. u. Hyg. des Auges*, September 8th, 1910.)
13. Stuelp: Salvarsan in Ophthalmology. (*Wochenschr. fuer Ther. u. Hyg. des Auges*, February 23rd, 1911.)
14. Uhthoff: Report on Salvarsan. (*Muench. med. Wochenschr.*, No. 48, 1911.)
15. Westhoff: Salvarsan in Ophthalmology. (*Wochenschr. fuer Ther. u. Hyg. des Auges*, May 25th, 1911.)
16. Wibo: Salvarsan in the Treatment of Affections of the Eye. (*XIV Congrès flamande des Sciences naturelles et médicales*, 1910.)

Shortly after Ehrlich announced the discovery of salvarsan, ophthalmologists began to "try out" the new remedy in cases of ocular syphilis. The earlier trials were made with due caution, for ophthalmologists had learned to fear possible disastrous ocular sequelæ following the use of certain of the newer arsenical preparations, notably atoxyl and soamine. There is recorded a number of well-authenticated instances of

retrobulbar neuritis eventuating in atrophy after the administration of atoxyl; and Ehrlich, fearing a similar untoward effect from arsenobenzol, warned against the use of the drug in any patient showing signs of optic neuritis or retinitis.

With the widespread adoption of salvarsan as a remedy in syphilis, it became important to determine whether this drug was likely to be followed by such sequelæ. Evidence on this point is as yet not conclusive. It is probably true, as pointed out by de Lapersonne and Levi, that there is "no need to exaggerate the dangers of salvarsan as regards vision." Contrasting the effect of atoxyl with that of salvarsan, these authors state that "atoxyl may give rise to true subacute retrobulbar neuritis manifested by definite signs, and comparable with the toxic neuritis, experimental or pathological, due to the effect of quinine, male fern extract, carbon bisulphide, etc. So far no such toxic neuritis has been described following the use of salvarsan. The latter's feebleness coupled with the fact that although given in massive doses, one or two only are administered, renders it much safer, for renal elimination quickly occurs."

Stuelp published one of the first reports of the use of salvarsan in syphilitic disease of the eye. The following is a résumé of his experience. One case of choked disc with oculomotor palsies, cured; one case of syphilitic optic neuritis, rebellious to mercury, cured; one case of keratitis apparently cured in eight weeks, then relapsed, and finally cured after a second injection of salvarsan; 3 cases of iritis (one gummatous), cured; one case of exophthalmus with abducens palsy (hereditary syphilis), cured; one case of syphilitic retinitis, cured; one case of optic atrophy, unimproved; 5 cases of interstitial keratitis, four unimproved, one temporarily improved, but relapsed. In a subsequent paper based on personal experience and the literature, Stuelp tabulates the results of the administration of salvarsan in ocular syphilis as follows:—

Cases	Rapid or good results Per cent.	No results or relapses Per cent.
Eyelid.	100	
Conjunctiva.	63	37
Cornea.	27	73
Sclerotic.	80	20
Uveal tract.	63	37
Neuroretinitis.	63	37
Eye-muscles.	33	67
Orbit and trigeminus nerve.	100	
411	66	34

Stuelp noted the occasional appearance of iritis, choroiditis, optic neuritis and ocular muscle palsy two or three months after salvarsan, given in the early stages of the disease. He concludes that salvarsan should be administered: (1) When a rapid effect is desired (primary disease or rapid loss of function); (2) in cases where mercury and the iodides are not well borne or are without effect. In all other cases "rely on older methods."

Occasional ocular complications observed were scotoma scintillans, transitory amaurosis, ptosis and increased tension. Stuelp believes that the sole contraindication to the use of salvarsan (from the ocular standpoint) is the presence of non-syphilitic retinitis or optic neuritis.

Nacht's experience includes 13 cases, of which ten were failures, three

successes. Of the ten failures, five were tabetic atrophies, the remainder parenchymatous keratitis, chronic iridocyclitis and vitreous opacities. The three successes were in iridocyclitis, paralysis of accommodation, and in unilateral paralysis of accommodation and the sphincter of the pupil. The success in the last-named case was only partial, as the paralysis of accommodation persisted after the pupil had regained its normal size.

Westhoff administered the drug intramuscularly in 6 cases. Of 3 cases of iritis, only one showed improvement. In one case of unilateral optic neuritis, the condition improved, but the hitherto sound eye became affected. In one case (choked disc) the patient became unconscious six days after the administration and remained so for five days; vision, which had only been slightly impaired, was suddenly lost, never to be regained. No improvement in a case of retinitis with hemorrhages. Westhoff remarks that "several of these patients improved after a return to the usual treatment."

Fifteen cases of tertiary ocular lesions (all giving positive Wassermann reactions) were treated with salvarsan by Wibo. Cure was effected in 2 cases of interstitial keratitis, and two of paralysis of the extraocular muscles. Slight improvement in several cases of chronic neuritis and retinitis.

Flemming reports the action of salvarsan in 180 cases of ocular syphilis from Greef's clinic. All gave positive Wassermann reactions. No special method was used exclusively. The dosage ranged from 0.3 to 0.7 gm. for adults; for children 0.08 gm. for each kilo of body weight.

Generally speaking, the drug improved vision. There were no signs of poisoning. Of 12 cases of interstitial keratitis, not one showed a definite cure four months after the administration; in some the inflammatory signs diminished, in others there was no improvement. Iritis, chronic iritis and choked disc were favorably influenced. No improvement in a case of sympathetic ophthalmia. No permanent results in paralysis of the extrinsic ocular muscles. Results in cases of optic atrophy and disturbances of pupillary action were negative.

Inequality of the pupils was noted by Flemming in 7 cases after injection, and accounted by him a secondary symptom of syphilitic infection.

Flemming states that the results of the treatment, with but few exceptions, were no more pronounced than those obtained by the recognized antisiphilitic therapeutic measures.

Frequently subjective symptoms disappeared quickly, while physical conditions did not change more rapidly than under mercury or the iodides.

Uhthoff found salvarsan of no avail in interstitial keratitis. Seeligsohn and Best met with similar disappointment. Michaelis reports a cure of a case of choked disc incident to cerebral gumma.

Twenty-one cases of ocular syphilis—iritis, keratitis, neuroretinitis, tarsitis, paralysis of the superior oblique, pupillary disturbances in tabes, and optic atrophy—were subjected to intragluteal injections of salvarsan by Schnaudigel. In 20 cases the treatment "succeeded admirably." In the light of his high percentage of successes it is not surprising that he concludes that "in arsenobenzol we have a most important addition to ophthalmic therapeutics." The sole failure was in a case of interstitial keratitis, commenting upon which Schnaudigel remarks that "owing to the indirect blood-supply of the tissue, the cornea is one of the most difficult parts of the body for the action of antisiphilitic remedies."

de Schweinitz and Shumway report the cure in ten days of a case of

iritis papulosa. Two cases of interstitial keratitis and one of optic neuritis rapidly responded to salvarsan administered by Denig, who suggests that a mercurial course prior to salvarsan may give still better results.

In the discussion which followed, Weeks mentioned having observed cessation of photophobia in interstitial keratitis following salvarsan, without any noteworthy clearing of the cornea.

In Coover's case of recurrent iritis, a rapid cure followed salvarsan. The drug was also efficacious in a syphilitic neuroretinitis and hyalitis which had failed to respond to mercury, the iodides and cacodylate of soda.

AMENORRHEA.

A REVIEW OF RECENT LITERATURE.

By HUGO EHRENFEST, M. D., of the Editorial Staff.

1. Carstens: Stem Pessary for Amenorrhea, Dysmenorrhea and Sterility. (*Journ. Amer. Med. Assoc.*, November 20th, 1909.)
2. Editorial: Hypersecretion of the Thyroid. (*Journ. Amer. Med. Assoc.*, December 24th, 1910.)
3. Gellhorn: Amenorrhea. (*St. Louis Medical Review*, August, 1910.)
4. Hoover and Marden: Case of Complete Amenorrhea. (*Surgery, Gynecology and Obstetrics*, March, 1911.)
5. Meirowsky and Frankenstein: Amenorrhea and Tertiary Syphilis. (*Deutsch. med. Wochenschr.*, August 4th, 1910.)
6. Rieck: Hitherto Unrecognized Mechanical Type of Amenorrhea. (*Muench. med. Wochenschr.*, March 16th, 1909.)
7. Rosenberger: Concerning the Etiology of Amenorrhea. (*Zentralblatt fuer Innere Medizin*, February 25th, 1911.)

A most satisfactory presentation of the question of amenorrhea is found in a paper of Gellhorn, from which the following quotations are taken:—

"Amenorrhea is merely a symptom, not a disease *sui generis*. The prognosis and treatment of amenorrhea will depend solely on the correct diagnosis of the primary affection. In the majority of cases we find the cause either in a local condition, *i. e.*, a disease or change of the genital organs, or in a general condition, which affects the entire organism. In a remaining minority of instances, the etiology, as far as our present knowledge goes, is obscure.

"Foremost among the local causes is the physiologic amenorrhea due to pregnancy. The second physiological cause is lactation, which leads temporarily to a hyperinvolution of the uterus and amenorrhea. In debilitated and anemic women, however, or when the period of lactation was unduly prolonged, the amenorrhea may persist as the result of an atrophy of the uterus.

"A comparatively large number of uterine atrophies with consecutive amenorrhea is due to pathologic processes in the puerperal uterus. Puerperal infections may produce a partial gangrene of the uterus by which a portion of the uterus wall is sequestered. Occasionally the patients recover, and the formation of scar tissue results in a complete destruction of the uterine mucosa. In other instances, parametric exudates, perimetric inflammations due to gonorrheal infection, extensive hematoceles, or hemotomas within the broad ligaments after extra-uterine conceptions, may compress the uterus to such an extent that the nutrition of that organ will suffer more or less severely. Finally, deep lacerations of uterus and cervix are liable to cause atrophy. Outside of the puerperium, endometritis and metritis of long standing can affect the uterus in a similar manner. Atrophy and amenorrhea have frequently been observed in vesical fistulæ, particularly in those in which a communication existed between the bladder and the cervix. Atrophy may also be produced artificially by a too strenuous curettage for endometritis, or after miscarriage. A similar unforeseen result was frequently observed when intra-uterine applications of chloride of zinc were the fashion, and in contemporary German literature a number of cases had been reported in which atmocausis, *i. e.*, the intra-uterine use of superheated steam, was followed by complete destruction of the endometrium.

"Diseases of the ovary do not produce amenorrhea except when every trace of ovarian tissue has been destroyed as, for instance, in ovarian abscesses in the wake of severe puerperal infections.

"The atrophic uterus differs greatly from an abnormally small or even rudimentary uterus which, from the very beginning, has been unable to menstruate. In this latter class of cases we have to deal with congenital malformations. It is readily understood why asplasia or hypoplasia of the uterus or ovaries must needs produce amenorrhea.

"There is another congenital malformation associated with amenorrhea which, however, does not come within the scope of this paper. In the cases of imperforate hymen or atresia of the vagina there is no amenorrhea *sensu strictiori*. Menstruation occurs regularly but the blood is retained within the upper portion of the vagina, the uterus, and the tubes, thus forming a hematocolpos, hematometra, or hematosalpinx respectively.

"The local causes of amenorrhea rank, as far as their frequency and practical importance are concerned, behind those which have their origin in some affection of the entire organism. It may be stated as a general proposition that a 'constitutional amenorrhea' may be found in almost all diseased conditions, acute or chronic, which make heavy demands upon the vital forces. Such a repression has always been regarded as a conservative effort on the part of nature to preserve the patient's strength.

"Of acute diseases, scarlet fever, articular rheumatism, and typhoid have been observed to cause amenorrhea which usually lasts through convalescence until health is re-established.

"Among the chronic constitutional affections, chlorosis is the most frequent to produce amenorrhea. Next in order is tuberculosis which may disturb the menstrual function even in the earlier stages of the disease. In Graves' disease, severe forms of syphilis and nephritis, in acromegaly and Addison's disease amenorrhea is frequently but not regularly noted.

"The authorities differ as to the effect of diabetes upon menstruation. Obesity exerts a marked influence. Kisch observed in 87 obese women amenorrhea 15 times; abnormally scant menstruation, 47 times; profuse menstruation, 20 times.

"Chronic poisonings of lead or mercury, morphinism and alcoholism occasionally produce amenorrhea.

"Amenorrhea, however, may occur without any apparent cause in perfectly healthy and normal women. The fact has been established that such women may even conceive; therefore, in these cases the function of the ovaries was not disturbed.

"How subtle the influences are, which may lead to amenorrhea, can be seen in women who lose their menstruation temporarily if they move from one place to another or change their daily occupation.

"When amenorrhea occurs in sisters or in members of one family, it seems difficult to exclude hereditary influences, perhaps on an atavistic basis. Merely psychic factors are at times at work, particularly in neurotic individuals. This is best illustrated by the interesting cases of 'nervous pregnancy' where either the intense desire for children, or the overwhelming fear of having conceived may produce not only all the subjective symptoms of gestation but amenorrhea as well. Common as these cases are, the mechanism of the causation of amenorrhea still remains within the realm of speculation—as, indeed, this whole chapter is in need of a great deal of elucidation.

"Any therapeutic effort must needs take the etiology of the individual case into consideration. *The whole question of amenorrhea, therefore, is primarily one of diagnosis.*"

In concluding his article with an extensive reference to the various dietetic, hygienic, local conservative, and surgical methods of treatment, Gellhorn emphasizes the self-evident fact that the choice of the most promising therapy in the individual case is dependent on a thorough examination not only of the genital, but of all the organs of the amenorrheic woman.

In recent literature we find many advocates of the apparently well-established theory of a close connection existing between the function of the genital organs and of the glands with internal secretion. An unsigned editorial in the *Journal of the American Medical Association* states that the absence of menstruation, after it once has developed, without pregnancy or acute or chronic disease, generally points to a diminution of the thyroid and ovarian secretions. If the patient is anemic, iron and ovarian extract should be the treatment. If the patient is not very anemic

and tends to put on weight, thyroid is the treatment. The dose should be small, not more than 3 grains of the dried extract once a day.

Rosenberger, on the other hand, dwells upon the intimate relation of anomalies of the hypophysis to anomalies in the function of the female genitalia, and also to glycosuria. Amenorrhea is common in acromegaly, and in probably one-half of all cases of amenorrhea complicated by glycosuria, in his belief, the underlying cause is an acromegaly. For this reason every case of amenorrhea demands our serious attention. It may be the first sign of a developing acromegaly and, in his opinion, stands to this disease in the same relation as struma does to Graves' disease.

The possible significance of syphilis in the causation of amenorrhea is not generally appreciated, although we find it mentioned occasionally. Meirowsky and Frankenstein observed in three women, aged from twenty-eight to forty-two, an amenorrhea persisting from six to eight years. They all exhibited characteristic symptoms of tertiary syphilis, and were subjected to a rigid mercury and iodide treatment which resulted in the return of the menstrual flow. In one of the women, the oldest, menstruation seemed to reappear in the vicarious type of an epistaxis. The authors suggest an antisiphilitic treatment in all amenorrheic women suffering from syphilis, on the assumption that this disease possibly may interfere with ovarian function.

An observation recorded by Hoover and Marden seems to support the possible hereditary influence also mentioned by Gellhorn. They saw a woman, forty years old, apparently in good physical condition, of average development and intelligence, who had never menstruated in her life, although she had given birth to eleven children. On further investigation the authors found that the patient's grandmother had never menstruated, that her mother had menstruated only at intervals of one or two years. Of the patient's children, one girl of fourteen years had not yet menstruated.

Rieck calls attention to another mechanical cause of amenorrhea, heretofore never described; but it may be stated that his article is not very convincing. He found in two girls, nineteen and twenty-one years old, the uterine body very small and of the infantile type. A circular dense band high up in the uterine cavity seemed to form a complete obstruction to the escape of the menstrual fluid. There are no definite diagnostic features which will permit the recognition of this unusual condition. In both cases it was accidentally discovered when through a vaginal incision the anterior uterine wall was split longitudinally and the uterine cavity laid open. In both cases the operation was followed by menstrual discharge; in one case remaining very scanty, in the other being free and normal.

Among the forms of treatment less favored by modern gynecologists we find the use of the stem pessary. According to Carstens, this method deserves a more general use. In his hands the stem pessary has yielded splendid results in the treatment of the following special types of primary and secondary amenorrhea:—

1. In the small, narrow infantile uterus in otherwise well-developed girls.
2. In a class of cases in which menstruation has been regular and normal for years, but at the age of thirty or thirty-five becomes scanty and skips, and is painful in unmarried women who take little exercise but work hard mentally. The uterus undergoes premature atrophy and is found small.
3. In women who live high, have good digestion, and become fleshy, the menstruation becomes scant and sometimes disappears entirely.

CORRESPONDENCE

PARIS LETTER.

THE FIGHT AGAINST CANCER.

By AUGUSTE A. HOUSQUAINS, M. D.

The Second International Conference for the Study of Cancerous Diseases, which held its meetings in Paris at the close of 1910, has just published a volume of the papers and reports which were read in the course of the meetings as well as the discussions which followed the reading of the papers.

In all the papers which were read the dominating note was that cancer is on the increase. At first one is tempted to attribute this increase to the means of clinical investigation as well as the laboratory methods which are to-day so perfect that the diagnosis of cancer is robbed of nearly all its difficulties and hence is easy of diagnosis. But even if we reason in this manner, it should not be forgotten that cancer is a disease that has seldom been overlooked by the patient himself; and that even if the diagnosis in the past was not of the surety it is to-day in the first stages of the evolution of the disease, it is a fact that its existence was so apparent in the terminal stage that only in exceptional cases was its presence positively ignored. Superficial reasoning, then, must give way before the onslaughts of truth: there are more cases of cancer to-day than there were fifty years ago!

This increase is striking in civilized countries; and with adults it occupies the second place among the causes of mortality, tuberculosis occupying the first place. What renders this statement particularly disquieting is that despite undeniable progress, the therapeutics has remained relatively powerless in combating the disease. In default of a curative treatment, which remains yet to be found, the fight against cancer should consist of a methodic utilization of all the adjuvant means which are at our disposal at the present time, and of a complete co-operation among the physicians who make a specialty, not only of treating but of fighting this disease, to disseminate the best possible means of keeping the disease in check. This co-operation should consist, as was so pointedly said by Castaigne at one of the meetings, of the following three propositions:—

(1) Medico-surgical services should be utilized to study all the forms of cancer, especially in cancer hospitals to which laboratories are attached.

(2) The organization of a society for the study of cancer, to which physicians and surgeons, who make a specialty of treating cancer, should belong, as well as the investigators who study cancer in the laboratories, so that the clinical and laboratory results may be discussed and some control placed on those which are not efficacious.

(3) A committee of publication which is dependent on the preceding society and which should publish, not only the discussions of the society, but also a journal that can be sent gratuitously to all French doctors, so that they may learn the progress which is being effected in the fight against cancer and be encouraged into registering their own cases; and also brochures destined for the public at large, to the end of refuting erroneous opinions which are now current in regard to cancer and which only too often are an obstacle to the rational treatment of this disease.

The most important point in the anti-cancerous armament, and the realization of which is of the greatest urgency, is without doubt the creation of special hospitals organized for the practical and scientific study of cancer. The object of these hospitals is to house all patients who can either be cured or temporarily benefited by operation, sometimes the treatment given in an outdoor clinic sufficing. Patients who cannot be benefited by surgical operation or any other therapeutic treatment should be kept in hospitals, if only to alleviate their suffering. By collecting and arranging the various observations resulting from a study of the cases; by making laboratory researches in parallel cases; by compiling statistics; scientific documents of the greatest value can be accumulated. As examples of this sort of institution we may cite the Middlesex Hospital of London, the endowment at Heidelberg due to the initiative of Professor Czerny, the Barnard (Free) Skin and Cancer Hospital of St. Louis, and the New York Skin and Cancer Hospital. The number of these hospitals should be increased in all civilized countries if we wish to obtain results worthy of so excellent an enterprise. In each of these hospitals four physicians should exercise the following functions: One for the surgical operations, one to give medical treatment, one to practise physiotherapy, and one to make researches in the laboratory. The close and constant collaboration of these four specialists is indispensable.

As regards the creation of societies to study scientifically the question of cancer, there has been one in France for the last three years, which is known as the Association for the Study of Cancer. The criticism which may be brought against this Association is that it is too restricted in its functions, since its publications are destined only for a small number of physicians, and not for physicians at large. That is to say, the work which is done has not the publicity which it should have. Now it cannot be gainsaid that only by dissemination of discoveries concerning the nature and treatment of cancer can a more intimate knowledge of the disease and its treatment obtain among all in the profession. To effect this, what is necessary is the publication of a periodical, which is exclusively for doctors, as well as the broadcast distribution of brochures written in so clear and simple a manner that the education of the public may be effected in combating above all the prejudices which obtain at the present time against the early treatment of cancerous affections.

Every doctor should, at the present time, consider the following rules as essential. In the first place, cancer cannot be treated successfully if an early diagnosis is made in a superficial manner. All the known methods should be brought into play to arrive at a positive diagnosis: hemodiagnosis, intradermal reaction, and the successive analyses of the urine. X-rays, the examination of cavities by the endoscopic method, and finally biopsy, complete the ensemble of necessary researches. Once the diagnosis is established, and this as soon as possible, one should have recourse to surgical intervention, which in the majority of cases is the best treatment in the first stages of the disease. For the operation to be

efficacious, it should be conducted rationally and with precision; it ought above all—and this is really the advice of the most enthusiastic operators—be helped out by adjuvant therapeutic measures. Pierre Delbet, in his treatment of cancer of the breast, begins by using deep radiotherapy; this is followed at the end of some days by globular extirpation of the breast and the painting of the wound with pure tincture of iodine. Finally, he places under the clavicle a tube containing at least three centimetres of bromide of radium, and this is left in place one or two days. Other adjuvant measures particularly applicable in inoperable cancers are fulguration, electro-coagulation, chemical dressing of the wound, serums, vaccines, and opotherapy. Apropos of analgesic medication, it is necessary to insist on the fact that the dose of morphine ought to be accurately gauged if one wishes the desired effect; otherwise this drug will only produce further inconveniences.

But even though physicians may know perfectly well the means which are at their disposal to combat cancer, this should not suffice; for above all what is of prime importance is this, that patients should be treated sufficiently early by these methods. To accomplish this it is absolutely necessary to educate the public and destroy the many prejudices which now obtain even among the best educated.

An interesting communication by M. Beclère contains popular instruction which should be spread at large. The following is the gist of his paper: Cancer is not a constitutional disease and it has not been proved that it is hereditary. In the beginning, it is a purely local disease. Although in its incipency it does not make great inroads into one's health, it nevertheless resembles a parasite which grows and multiplies. Later on very small particles detach themselves from the original tumor and colonize in organs which were not attacked in the first place.

In a great number of cases, cancer is perfectly curable. To achieve this end it is necessary completely to eradicate the growth by a radical operation. It is never the case that one operates too soon. Unfortunately, since the disease manifests itself in the beginning only by a small non-painful tumor, it often happens that it remains unnoticed, and in this the danger lies. It is necessary to make an examination at once, and this should never be done except by an experienced physician. No matter what is said on behalf of other measures for removing cancer, it must be admitted that surgical interference is the best means we have at hand to destroy this growth without pain and without danger; but though this statement cannot be controverted, mention should be made here of certain other agents, such as the actual cautery, chemical caustics, *x*-rays, radium, and electricity, which may be used with good effect. The early employment of divers methods of local treatment, alone or combined, is really the only way, in a great number of cases, of causing complete destruction of a cancerous growth before it becomes general; that is to say, the definite cure of a disease that is purely local in the beginning.

Such, then, is the aspect of the question of the fight against cancer. One can readily see that the rôle of the physician is not only that of a therapist, but also, to a great extent, that of an agent through whom the popularization of knowledge may do much to prevent mortality, pain, and the employment of means which are deplorably ineffective.

September 10th.

THE NAUHEIM TREATMENT.

BAD NAUHEIM, September 18th, 1911.

Among the many watering places of the German Empire, Bad Nauheim has become the Mecca for patients suffering from cardiac diseases. The city lies 448 feet above sea-level and is beautifully situated on the eastern slope of the Johannisberg, a spur of the Taunus Mountains. Over 30,000 persons visited this place last year and an equal number have returned this year. The bath-houses are comfortably arranged with large entrance- or waiting-rooms in the centre, while the individual bath-rooms run out on each side, leaving an ornamental court with fountain and garden in the centre. The Grand Ducal Bath Administration has made a special provision for physicians, which entitles all physicians with proper credentials to the use of all the baths, free of charge, excepting more than one *Stromsprudelbad*. The *Kurtax*, which patients pay for the use of the baths and for the entertainments given at Bad Nauheim, is not levied on physicians.

The curative resources of this Spa consist primarily of three springs. Nos. VII, XII and XIV, which rise from a depth respectively of 530, 600, and 696 feet. Owing to the strength of the CO₂, these springs burst into the air, foaming like champagne. About 6,000 baths can be given daily. Spring No. VII has a temperature of 29.9° C., 2.16 per cent. of salt and 2,277 c.cm. of entirely free carbonic acid. This is the richest spring in carbonic acid. The composition of No. XII is 4.34 per cent. of salt, 1931 c.cm. of free carbonic acid, and the temperature is 34.4° C. No. XIV has a temperature of 32.2° C., 2.82 per cent. of salt, and 1715 c.cm. free carbonic acid. The baths in general use are the thermal, thermal-sprudel, sprudel, sprudel-stream and brine baths. The thermal bath is supplied through a spring, the water of which has been collected in a large outside reservoir where a portion of the carbonic acid gas evaporates and some of the salts precipitate, while the water for the thermal-sprudel bath is collected in closed reservoirs and stored therein for use as needed.

The sprudel bath is prepared from the water of the springs, which is brought through conduits leading from the upper tube direct into the bath; while the sprudel-stream bath is the same except that it has a special contrivance by which there is a continuous in- and out-flow of the waters. The great advantage of Bad Nauheim over the bath procedures in homes lies in the fact that the bath can be given in every desired degree of dilution with or without carbonic acid, changed at will as regards the salt contents, and prepared at any temperature needed. Owing to this manifold variety, it is possible to prescribe baths just as they are suited to the temporary morbid condition, and therefore to the individual necessity of the patient. Twenty to thirty baths of from eight to twenty minutes' duration are usually prescribed during a course of treatment. The temperature is gradually lowered from 33° C. to 30° C., and the bath increased in strength according to the indication of each patient's condition. For relative cardiac insufficiency the following course is suitable:—

- 4 Thermal baths, temperature 33° C.
- 4 Thermal baths, temperature 32.5° C.
- 3 Thermal-sprudel baths, temperature 32° C.
- 4 Sprudel baths, temperature 31.5° C.
- 4 Sprudel baths, temperature 31° C.

The course of treatment lasts from four to five weeks, the patient leads a quiet life, a rest of an hour to an hour and a half after each bath is ordered in the recumbent position. Fluids are restricted to one and a half pints in twenty-four hours, and in general a severe dietetic regime is enforced. At the conclusion of the bathing procedure, the patient is sent to some health-resort for a rest of two or three weeks, the so-called after-cure.

Now as to the physiological action of the carbonic-acid bath in general. The action of the carbonic-acid bath on the heart has been compared by Schott with that of severe gymnastic exercise. It is, therefore, a strenuous and not a mild method of treatment, and requires a certain amount of reserve force. The primary action is the same as in all cold-bath procedures, and there is a general rise of blood-pressure if the temperature is below 33° C. This rise takes place rapidly and lasts for half an hour after the bath. The superficial blood-vessels contract stronger in the CO₂ bath than in the ordinary water bath with equal temperature. The dilatation of the deep vessels is only slightly compensatory; and accordingly the increased pressure is maintained and remains uninfluenced by the dilatation of the small peripheral vessels and arterioles of the skin, which produces in the bath a slight erythema of the skin. The gas bubbles cover the skin densely, producing a subjective sensation of warmth, which feels very agreeable and prevents the patient from being uncomfortable from the lowered temperature. The pulse becomes fuller, respiration deeper, and through the stronger contractions of the heart the superficial organs are better nourished as well as the heart muscles themselves.

As this form of treatment calls forth an extra amount of heart energy, it is not advisable to combine the bath with the gymnastic exercises as carried out under the Schott and Oertel method of treatment. Each case requires under this form of treatment careful and painstaking ordination and observation by a competent physician.

OSCAR H. BENKER, M. D.

A PROTEST FROM PROFESSOR CLOETTA.

ZURICH, August 21st, 1911.

To the Editors, Interstate Medical Journal:—The June issue of the INTERSTATE MEDICAL JOURNAL contains an article by Drs. Boos and Lawrence with statements relative to digalen, which cannot be permitted to pass unchallenged, as they are not only incorrect, but also show a tendency to promote unfavorable prejudice. I have submitted digalen to physicians for trial; and not until after such eminent clinicians as Naunyn and Senator had made use of it for one year and assured me of its excellent properties, did I permit of its being placed on the market. Of course, even at the present time we are desirous of professional criticism, and such we have in the way of 250 original articles. Of these, 240 (96 per cent.) strongly advocate the use of digalen. Of all these latter works Drs. Boos and Lawrence take no notice, and even do not acquaint their readers with their existence. Instead of an impartial and objective report concerning *all* published articles regarding digalen, the authors mention only some 15 reports for the purpose of proving digalen a worthless product.

I owe it to my self-esteem, relative to my work of many years in the realm of digitalis therapy, as well as to the early investigators who did pioneer clinical work, not to ignore such palpably prejudiced statements.

What now is the import of the adverse articles mentioned by Drs. Boos and Lawrence? Among them we find, for instance, one by Prof. Kottmann, the result of clinical observations made in Prof. Sahli's clinic, which is pretended to show that the toxic dose of digalen is closely allied to the therapeutic. As a matter of fact Kottmann said the very opposite, inasmuch as he emphasized that in spite of the large doses given formerly, digalen never caused any deleterious results. The same therapeutic results, though, could have been obtained by much smaller doses. In other words, Kottmann stated just the opposite of what the authors claim—to wit, that in spite of the large doses formerly administered digalen never produced any alarming symptoms; smaller doses, however, would have been productive of the same clinical results. Thus we see the "therapeutic dosage latitude" of digalen is very wide. The unfavorable relation between therapeutic and toxic dosage reported by the authors is referable to strophanthin and not to digalen; which fact Kottmann expressly states on p. 308, relative to a lethal case, following an injection of strophanthin. It can only be inferred that the authors have confused these statements. As a further proof of the fatal properties of digalen the authors quote Teichmann. Now, as a matter of fact, in his concluding sentences, Teichmann says: "Digalen is a complete substitute for all the galenical digitalis preparations, which as a matter of fact it excels in its standardization and readily regulated dosage." He cautions against its use in cases of advanced arteriosclerosis, which of course is quite self-evident. His last remarks are literally: "The intravenous injection of digalen is an inestimable addition to our knowledge of therapeutics. It is indicated whenever a rapid effect is desired." According to Drs. Boos and Lawrence these remarks stamp digalen as a worthless preparation. Veiel, who is also quoted as saying that digalen could not be considered a desirable substitute for our supply of heart remedies, states in his concluding remarks that while digalen possesses no marked advantages over the known heart remedies, in acute cardiac weaknesses it forms a not to be ignored remedy, the administration of which should not be neglected. I am of the opinion that this puts rather another aspect on the statements of Drs. Boos and Lawrence. It also, I think, demonstrates the fact how "critical" the gentlemen were in making statements, especially when one considers how careful they were in selecting unfavorable statements embodied in 15 articles and totally ignoring the 240 favorable reports.

Even if the authors mention Eichhorst as being opposed to digalen, I am able to say that in an address delivered before the Medical Society of Zurich (*Vide* Protocol of the Medical Society of Zurich, 1911, published in *Correspondenzblatt für Schweizer Aerzte*, 1911) he states: "Of all substitutes for digitalis, after years of experimental work in our clinic, there is *none* which has proven its value to such an extent as digalen." Of this statement of Prof. Eichhorst, the authors were undoubtedly ignorant. I simply mention it to show that if Prof. Eichhorst formerly entertained unfavorable ideas concerning digalen, he has after several years' careful study and observation arrived at a totally different conclusion.

As the three examples selected by me from the work of Drs. Boos and Lawrence are not even properly quoted by them, I will assume for their

benefit that they have allowed themselves to arrive at erroneous conclusions owing to incorrectly quoted statements. As regards the chemical aspect, I would beg all, who have not themselves labored in this field, to moderate their views and judgment regarding this most difficult chapter in glucosidal chemistry. I trust that at no late date the many erroneous ideas still present in the minds of many will have been banished and the entire digitalis chemistry be revealed to us. As regards experimental work relative to the action of digitalis preparations on the healthy animal, I cannot attach importance, if the results obtained shall be used to demonstrate the therapeutic action upon the sick human subject. The action of digitalis upon a diseased human heart is entirely different from that on a normal and healthy animal heart. This is a fact recognized by all physicians, and one need only remind oneself of the strongly toxic effect of strophanthin tincture upon the frog's heart, and remember how far less efficient it is than digitalis, when applied to the human. It is to be regretted that there are still numerous pharmacologists who have been unable to rid themselves of these erroneous ideas. As a result of this the commission, appointed for the Swiss Pharmacopeia and composed of experienced physicians, definitely refused to fix a physiological dosage for digitalis preparations.

Yours very truly,

M. CLOETTA.

DIAGNOSTIC AND THERAPEUTIC NOTES.

TOLERANCE OF INFANTS FOR MORPHINE.—Wichura (*Muench. med. Wochenschr.*, 1911, No. 30). The writer reports a case of morphine poisoning observed in an infant of three months. By mistake the mother gave the baby $\frac{1}{3}$ gr. of the drug, and called the doctor when untoward symptoms were noticed. The child revived in the course of twenty-four hours. Wichura believes that the maximum dose of opium for children, as stated in the textbooks, is too low, and that they are not nearly as sensitive to the drug as is commonly supposed. He has often given codeine, $\frac{1}{3}$ gr., to a child of one or two years for an irritating cough without any appreciable influence.

TREATMENT OF GONORRHEAL JOINTS WITH TR. IODI.—Hildebrand (*Berlin. klin. Wochenschr.*, 1911, No. 31). The author has treated a number of gonorrheal knee-joints and one ankle according to this method with uniformly good results. The cases selected were those of moderate severity, no very extreme case having yet been subjected to this plan of treatment. Five grams of the tincture are injected directly into the joint. The injection is followed by decided swelling, but this subsides in a short time. In all the cases thus far, the mobility of the joint after treatment has been unimpaired.

DILATATION TEST FOR CHRONIC APPENDICITIS.—Bastedo (*Amer. Journ. Med. Sciences*, July, 1911). There are many cases of dyspepsia, so-called, in which the stomach function is normal and in which our suspicions turn toward the appendix without however being able to elicit any definite signs of disease in that organ. Palpation may show little or no tenderness over the appendix region, or if the tenderness seems marked here we are puzzled by finding points of equal tenderness in other regions of the abdomen. Bastedo has made use of the following manoeuvre: With the patient in the recumbent position a colon tube is passed eleven or twelve inches into the rectum. Air is injected through this by means of an atomizer bulb. If appendicitis exists, pain and tenderness to pressure develop at McBurney's point as the colon distends. Bastedo has found the test quite useful in the differential diagnosis between uterus-adnexa inflammation and appendicitis. In the former condition the tenderness occasioned by the dilatation of the colon is less acute, lies lower down, and extends toward the middle line.

BLOOD ALTERATIONS IN ACHYLIA GASTRICA.—Waledinsky (*Deutsch. med. Wochenschr.*, 1911, No. 35). The writer reports his observations

on 8 cases of achylia gastrica. In all these the blood showed, if not an actual leucopenia, at least a leucocyte count which was at the lowest limits of normal, the average for the 8 cases being 5,400 leucocytes. The blood was taken in a uniform manner,—in the morning before the ingestion of food. The results of the differential counts were especially interesting, and if confirmed by other examiners will be of considerable diagnostic aid. Thus it was found that the neutrophilic leucocytes were markedly diminished, the average being 48 per cent. instead of 70 to 75 per cent., and the small lymphocytes were correspondingly increased. The percentage of basophilic and eosinophilic cells showed no variation from the normal. These results were found in cases of uncomplicated achylia as well as those accompanied by diabetes, cirrhosis of the liver, Banti's disease, chronic lead poisoning, and chronic nephritis. Gastric carcinoma, which often figures in the differential diagnosis, usually shows an increased number of leucocytes and a percentage of neutrophilic cells at or above the normal.

THE CONCENTRATION OF THE SODIUM CHLORIDE SOLUTION USED IN SALVARSAN INFUSION.—Wehner (*Deutsch. med. Wochenschr.*, 1911, No. 33). Formerly Wehner used the usual strength of sodium chloride solution, 0.9 per cent., in his intravenous injections of salvarsan. The injections were very often followed by fever and sometimes by chills and vomiting. Lately he has been using the sodium chloride in a concentration of 0.6 per cent., and the injections have been followed by no fever or only by a very inconsiderable rise without any disagreeable subjective symptoms. He recommends the weaker solution as far preferable.

THE CHOICE OF TUBERCULINS.—Bluemel (*Muench. med. Wochenschr.*, 1911, No. 34). The number of tuberculin preparations on the market is truly bewildering. Many of them, says Bluemel, have absolutely no therapeutic value, and of those which are really active, the difference between the various ones lies simply in the concentration and absorbability of the preparation. The Koch tuberculin and the tuberculin of Landmann accomplished everything that lies within the power of tuberculin therapy, and are to be recommended because they are easily obtainable, and of standard quality. The most important factor in this method of treatment still remains—the skill and intelligence of the physician. He must be able properly to graduate the dosage and to recognize harmful effects of too large or too frequent dosage. In proper hands the author believes the tuberculins have a decided place in the treatment of tuberculosis, but he reminds the reader that a faulty application can make the best therapeutic agent useless.

BOOK REVIEWS.

DISLOCATIONS AND JOINT FRACTURES. By Frederic J. Cotton, A. M., M. D., First Assistant Surgeon to the Boston City Hospital; Assistant Professor of Clinical Surgery in Tufts College Medical School, Boston. With 1201 illustrations. 330 drawings by the author. Philadelphia: W. B. Saunders Co. 1910. Price, \$7.50.

It is Dr. Cotton's modest statement that his book is made on the theory that no man can do more than present, with pen and brush, what he himself knows from instruction, reading, and personal observation, in as direct a way as is possible. Taking this as a text for his work, he has proceeded in a most direct manner to produce the best book which has ever appeared on the subject of dislocations and joint-fractures. To say the best book is perhaps drawing a long ball, but adherence to this statement has been strengthened in the reviewer's mind as he has become more intimately acquainted with Cotton's book and has more fully appreciated the author's attempt to get out a work which would represent his own ideas and personal experiences. The fact that all the illustrations have come from his hand, and, furthermore, the fact that the illustrations are actually a part of the text illustrating the author's ideas most graphically, are enough to endear the book to anyone who appreciates originality. In this textbook we have none of the old recopied photographs and woodcuts which date back to Malgaigne and Hamilton, but we have instead such illustrations as make the mechanics of the conditions described clear, and such as would give a good idea of modern treatment. Cotton says in his introduction that he originally planned his work as a treatise on dislocations. This plan was abandoned, and for it was substituted the more general title of dislocations and joint-fractures,—a very wise change for the reason that dislocations rarely present themselves to the medical practitioner as such, but rather as injuries to or near the joints, and again dislocations, as such, as the author says, "neatly and academically classified with all the reduction schemes thrown in could be treated in very few pages. This is not so of injuries to and about the joints; these injuries constitute one of the most disastrous fields of surgery, both to the patient and to the surgeon. They are prolific of discontent, disaffection, and legal action." Cotton's book is the first that has treated adequately this dangerous field from a modern point of view, with the possible exception of Stimson's, which covers fractures in general. His work may be said to be a summary of the subject based on his personal experience, and on this much emphasis should be placed. Cotton has not tried to reproduce all that has been written on this subject; he has not gone over the voluminous literature and appended the notes of literary references, but has given us what he terms, "a reaction from benumbing German scholasticism." What doubts he may have entertained, as to the advisability of producing a book in this manner, must surely have been dispelled ere this, for his work has that indescribable charm of being fresh and presenting a personal point of view. To the general practitioner or surgeon who does not look upon himself as an expert in the treatment of joint-fractures, this book, which represents Dr. Cotton's own experience, will prove a most valuable friend.

JOINT TUBERCULOSIS. By Leonard W. Ely, M. D., Consulting Orthopaedist to the County Hospital; Attending Orthopaedist to the Children's Hospital, Denver, Colo.; Member of the American Orthopaedic Association, etc. Illustrated. New York: William Wood & Co. 1911. Price, \$2.50.

The work done by Ely on tuberculous joints is well known to those especially interested in the subject. The book under discussion is a compilation of his already published papers, putting them in sequence and polishing them off a bit into well-rounded book form. The value of this monograph is such that it is hard to estimate it in moderate language. Heretofore the works in English

on this subject were those of Watson-Cheyne and Senn, both long since relegated to the limbo of the obsolete. The books in French and German are full of repetitions and much antiquated lore, furnishing a conglomerate mass of ill-assorted observations, and very few facts; indeed, being largely clinical in their importance. Ely must have been struck by this neglect, for he has turned to his work in a most systematic and energetic manner, and has given the subject what it needed—namely, a thorough overhauling by some strong hand, and a standardization of the observations and theories that have gone before. This assault is first made on the pathology, and he has cleared away many cobwebs and superstitions. The facts gleaned have been applied to the clinical methods in use, the empirical nature of which is all too sadly evident. Thus he has brought the subject of joint-tuberculosis to book, placing it upon a respectable basis, and a modern one.

The method used was to study diligently, without preconceived notions, a large number of specimens, and associate the clinical findings with those of the microscope. The cases which had started from a bone-focus were first investigated, then those of apparent synovial origin,—that old, much-discussed question. Ely has thrown new light upon it; he has found that the disease may start in the synovial membrane; that in many cases there is a bony involvement; but this involvement depends upon the presence of red marrow in the bone. The synovial membrane is vulnerable where it is thrown into ridges and folds, and possesses fringes and recesses; these conformations making the membrane, which is of the lymphatic system, as liable as are other connective-tissue structures to tubercle bacillus invasion. It would be necessary to quote the entire chapter on pathology to give an idea of its clarity and significance. Though it may seem radical, positive, or even revolutionary, it has the mark of careful scientific observation and is expressed in no mincing manner.

The pathology being thoroughly gone over, the author makes his clinical deductions, and these cannot be passed over lightly. Surely a systematic set of ideas is needed by anyone who undertakes the treatment of these conditions. What Ely has given us here is a working basis for treatment, backed up by what he himself has seen under the microscope.

He remarks that no one could imagine one surgeon neatly removing the diseased appendix, while another curetted the organ, and a third treated all cases of appendicitis expectantly; and he points the deadly parallel here to the surgical treatment given tuberculosis of the joint. The object of all treatment is to deprive the tuberculous joint of function. In children, operations which have for their object radical removal of tuberculosis are not to be done. In adults, where practical, the joint should be made stiff. The attempt to remove tuberculous foci with a curette is similar to attempting to remove a pea with a steam-shovel. On one thing he lays special stress: secondary infections after operation must be absolutely avoided.

Perhaps the most creditable thing in Ely's work is his earnestness, since his book speaks with a decided tone of conviction.

THE HUMAN ATMOSPHERE. Or the Aura Made Visible by the Aid of Chemical Screens. By Walter J. Kilner, B. A., M. D. Cantab., M. R. C. P., etc., Late Electrician at St. Thomas's Hospital, London. Illustrated. New York: Rebman Co. Price, \$4.00.

This book may properly be considered from three standpoints—the descriptive, physical and medical. The review, however, deals only with such physical theory as is offered in mechanical explanation of the phenomenon described. That the facts stated are of diagnostic value will be evident, but the reader will be the best judge as to the degree of their value.

The author describes a method of making visible a characteristic atmosphere or *aura* surrounding the nude human form by placing the subject about a foot in front of a suitably prepared dark background in a faint, uniformly diffused sunlight, and viewing him through one of the so-called *pole* screens, or first looking through a darker screen at the light and then at the patient without a screen. The "Spectan ranine" screens consist of two parallel plane plates of glass containing a solution of dicyanin or carmine in alcohol, a few millimetres in thickness. The body is then surrounded by a visible cloud consisting of three parts. Next the body is the *Etheric Double*, a dark band about a quarter of an inch wide and following the contour of the body. Next is the *Inner Aura*.

This is the densest portion of the "atmosphere" and follows the contour, blending with the etheric double and having a generally uniform width of about three inches. Outside this is the *Outer Aura*, characteristic of the person ob-

served. The author concludes that the etheric double is transparent and "does not contain any material." The structure of the inner aura is described as granular and striated. The outer aura "consists of a faint cloud and appears entirely structureless, capable of being 'illuminated but not luminous.'" Occasionally the author speaks of the *Ultra Outer Aura* of a patient possessing an unusually extensive aura. The aura is generally referred to as bluish or gray tinged with blue, and in the outer aura are observed *rays*, varying in color and structure.

The physical theory, or mechanical explanation offered by the author, is vague and not at all logical. In fact, quotations from various passages may best serve the purpose of getting at the "explanation," if the book may be regarded as offering one in the physical sense. On p. 93 is found: "The second theory, most probably the correct interpretation of the aura, is that it consists of a force emanating from the body, which like all forces, is invisible in itself, but which becomes perceptible by means of its action on the ether or atmosphere. Whether this supposition is true or not, it certainly deserves careful consideration." Again on the same page: "Fortunately, magnetism, radioactivity and electricity will supply three different kinds of force all producing analogous results; and they can be seen under conditions similar to those that make the human aura visible." On p. 99: "It is more than probable that the force giving rise to the human aura is quite distinct from the above three." On p. 101: "We are compelled to the conclusion that there must be two forces, one which originates in the inner aura, to be called No. 1 *Auric force*; another producing the outer aura, to be termed No. 2 *Auric Force*. . . . No. 1 *Auric force* acts apparently very intensely within a prescribed area, and is, to a certain extent, under the influence of the will." (!) . . . "No. 2 *Auric force* is certainly more mobile and has a wider range of action than No. 1; and, as far as has been determined is entirely independent of the *will power*."

It is obvious that in the modern physical sense we have here no explanation of the existence of the aura, and the problem of its visibility is treated in the same vague way. On p. 108: "Under the circumstances we may safely conclude that individuals who can perceive the human aura and the haze around magnets, etc., receive their power not from keenness of sight, but from a faculty to see rays that are not included in the ordinarily visible spectrum."

At the bottom of p. 112: "The following remarks are completely hypothetical and without proof, but we offer them in default of any other explanation, and ask our readers' kind forbearance if they disagree with them. We do not think an increase in the visual purple alone would be sufficient of itself to account for the perception of the aura, although it is quite possible that it may be some augmentation in the visual purple. It is more probable that there is some change in its constitution which, after a time, by the continuous use of the spectan ranine screen, becomes fairly permanent, and that this alteration enables a person to apprehend rays a short distance beyond the ordinarily visible spectrum."

In conclusion, it may be remarked that the phenomenon described above is of extraordinary interest from both the physical and medical standpoints, but that the explanation offered is entirely inadequate and speculative.

PATHOLOGICAL TECHNIQUE. A Practical Manual for Workers in Pathological Histology and Bacteriology, including Directions for the Performance of Autopsies and for Clinical Diagnosis by Laboratory Methods. By Frank Burr Mallory, A. M., M. D., Associate Professor of Pathology, Harvard University Medical School; Pathologist to the Boston City Hospital; and James Homer Wright, A. M., M. D., S. D., Director of the Pathological Laboratory of the Massachusetts General Hospital; Assistant Professor of Pathology, Harvard University Medical School. Fifth Edition, revised and enlarged, with 162 illustrations. Philadelphia and London: W. B. Saunders Co. 1911. Price, \$3.00.

This book is so well known and has so wide a circulation that to call attention in any special way to the fifth edition would be a matter of supererogation. In the latest edition all further knowledge as regards technique is noted, and just as in the previous editions there is no letting-up of the writers' hold on all those sources of information which have always been their chief characteristic. But though this can be admitted freely, it is with some surprise that the reviewer observes that the important new additions to our knowledge of the nature of the tubercle bacillus have been overlooked. Reference is here made to Much's method, which is superior to all other methods and does away with

the use of antiformin in many cases. The salient feature of Much's method is not its practicability in changing the tubercle bacillus from a bacillus to a congener of the actinomycotic class and similar organisms which cause infections other than the tuberculous sort, but its value in showing the character of the tubercle bacillus in any tuberculous material by the presence of branching chains of tubercle bacilli, the granular structure of the bacilli, and the breaking up of them into single granules, cultivated from a bacillus growth and from inoculation causing infection. On the practical side, Much's method is superior inasmuch as a larger number of bacilli may be demonstrated than by carbolfuchsin, and since other methods only too often prove futile in finding bacilli. And again by this method, granular bacilli may be seen, even the granules themselves. But what should not be forgotten is that antiformin merely preserves the bacilli and granules, and that carbolfuchsin is not capable of staining them.

THE TREATMENT OF SYPHILIS WITH SALVARSAN. By Dr. Wilhelm Wechsellmann, of Berlin, Medical Director of the Skin and Venereal Disease Section, Rudolph Virchow Hospital, Berlin. With an Introduction by Professor Dr. Paul Ehrlich, of Frankfort-on-the-Main, Director of the Royal Institute for Experimental Therapeutics, Frankfurt. Only Authorized Translation, by Abr. Wolbarst, M. D., of New York. Consulting Genito-Urinary Surgeon, Central Islip State Hospital; etc., etc. With 15 Textual Figures and 16 Colored Illustrations. New York: Rebman Company. Price, cloth, \$5.00.

This excellent book is a translation by Wolbarst of the original German work of Wechsellmann upon the treatment of syphilis with salvarsan. It is based upon the results of 1,400 cases treated by Wechsellmann, and contains all the essential features of a large clinical experience, scientific methods of investigation, and broad, conservative judgment. The translator has attempted to adhere, as far as possible, to the original text. Besides the original work of the writer, the literature from which the author draws is almost entirely foreign, which, otherwise, would be beyond the reach of most readers of English. The translation, however, has incorporated a list of American and British observers who have either had some personal experience with the new remedy or have established or correlated the writings of foreign contributors. Salvarsan is considered from every point of view, and the text is richly illustrated with colored plates showing its untoward by-effects as well as favorable results. It embraces the literature on this subject until February, 1910, and consequently gives the most reliable status of this new preparation up to that date. It is a book which throws many important side-lights on this very interesting disease, as well as one which represents the scientific observations of one of the foremost dermatologists having the most extended experience with this new remedy.

THE TREATMENT OF DISEASE. A Manual of Practical Medicine. By Reynold Webb Wilcox, M. A., M. D., LL. D. Third Edition. Thoroughly Revised and Enlarged. Philadelphia: P. Blakiston's Son & Co. 1911. Price, \$7.50.

Most textbooks of medicine give all too little space to the consideration of therapy. The present work aims to correct this by devoting itself especially to this phase of the science. More than 40 new sections have been added since the last edition. The author has not confined himself to infectious diseases usually met with in the United States, but has included those of other countries and climates.

HANDBOOK OF THE SURGERY OF THE KIDNEYS. By W. Bruce Clarke, A. M., F. R. C. S. Senior Surgeon to St. Bartholomew's Hospital. With 5 plates and 50 illustrations in the text. New York: Oxford University Press. 1911.

Clarke presents in this "Handbook of the Surgery of the Kidneys," an excellent work on the subject of renal surgery dealing mainly with personal observations and experiences coupled with careful statistical consideration. The book is divided into ten chapters which comprise the normal and abnormal anatomy of the kidney and ureters; methods of examination; operation on the kidney; injuries of the kidney and ureter; movable kidney and its results; hydronephrosis and its causes; stone in the kidney and ureter; tubercular diseases of the kidney; pyelonephritis and other infective conditions of the kidney, including perinephritis; tumors of the kidney and suprarenal body.

Each chapter has a condensed description of the subject and leaves but little to

be desired. The author throughout the work brings out a certain important point relative to a particular phase of renal surgery and follows it with an illustrative case in such a manner that the whole book reads like a novel. The chapter on differential diagnosis is particularly good, and takes up the various methods of differentiation of renal tumors from other abdominal tumors and from each other.

It is encouraging to see that he has had such favorable results with vaccines in affairs of the kidney and bladder. His results have been excellent with colon vaccine. It is an extremely valuable book and a very refreshing one to read.

DISEASES OF THE STOMACH AND INTESTINES. By Bordman Reed, M. D., Member of the American Medical Association, Consulting Gastro-Enterologist to the Pottenger Sanatorium, Monrovia, Cal., Late Professor of Diseases of the Gastro-Intestinal Tract, Hygiene and Climatology in the Department of Medicine of Temple University, etc. Illustrated. Third Edition, thoroughly revised and largely rewritten. New York: E. B. Treat & Co. 1911. Price, \$5.00.

The third edition of the well-known book on diseases of the stomach embraces all the new methods available in diagnosis and treatment of diseases of the digestive system from the standpoint of our present knowledge. As a one-volume edition it practically meets the requirements of such a text. Such subject-matter, as history of individual diseases, speculative discussion as to the mooted points of etiology, pathology, bibliography, etc., has, of necessity, been omitted. It exhibits as a basis much knowledge obtained from an evident extended clinical experience. Considerable space is given to recent innovations in the way of therapeutic resources for diseases, i. e., electricity, x-ray, violet rays, radium, manual and hydrotherapy; and approved hygiene and dietetic measures are consigned to their individual place in each chapter. It is a plain and unpretentious but practical clinical guide for diagnosis and treatment of diseases in question, and should continue to excite interest and approval in students of all branches of medicine.

A PRACTICAL TREATISE ON OPHTHALMOLOGY. By L. Webster Fox, M. D., LL. D., Professor of Ophthalmology in the Medico-Chirurgical College; Ophthalmic Surgeon in the Medico-Chirurgical Hospital, Philadelphia, Pa.; Member of the Army Reserve Medical Corps, etc. With six colored plates and three hundred illustrations in text. New York and London: D. Appleton & Co., 1910.

Dr. Fox has styled his book "A Practical Treatise on Ophthalmology" and the adjective well indicates the general character of the work.

Two courses are open to the author who writes a single volume treatise on diseases of the eye; he may, in a general sense, cover the entire subject, in which case many important topics will be dealt with too briefly; or, he may, by judicious selection, limit himself to such topics as are essential to one seeking a general knowledge of the subject. Dr. Fox has wisely chosen the second method. The treatise, up-to-date and readable, clearly reflects the author's forceful personality. Special prominence is given to methods of treatment and operations originated or amplified by the author. The book is copiously illustrated.

A Festschrift in Honor of Wilhelm Waldeyer on the Fiftieth Anniversary of His Doctor's Degree. *Archiv fuer Microscopische Anatomie*, 87 Band, 33 Tafeln und 96 Textfiguren mit einem Bild von Waldeyer. Bonn: Friedrich Cohen. 1911.

This book of 800 pages contains contributions from authoritative workers in the various branches of medicine and natural science. That it is well worth reading goes without saying, since it contains some twenty-five articles that have all the outstanding qualities which one expects to-day in contributions of a scientific nature. The most exacting student may read this *Festschrift* with profit, and even he, who has not the scientific spirit developed to this extent, will find enough of interest in the pages to hold his attention for long. This matter of *Festschriften*, so sedulously prosecuted by the Germans, deserves the highest commendation, not only on account of qualities which are unusual, but on account of the graciousness which is expressed by the writers in contributing their best thought to a book that is commemorative of the life-work of a teacher, whose message to the world should not be lightly passed over.

VETERINARY BACTERIOLOGY. A Treatise on the Bacteria, Yeasts, Molds, and Protozoa Pathogenic for Domestic Animals. By Robert Earle Buchanan, Ph. D., Professor of Bacteriology in the Iowa State College of Agriculture and Mechanic Arts, Division of Veterinary Medicine; Bacteriologist of the Iowa Agricultural Experiment Station. With 214 illustrations. Philadelphia and London: W. B. Saunders Company. 1911. Price, \$3.00.

This book is one of high interest, since it shows that the course of the infectious diseases of domestic animals is almost identical with what takes place when these diseases attack human beings. But though this is striking, what should not be forgotten is that the character of the infections in man and animal is similar only in a limited number of instances; in the majority there is no resemblance. While this book has been specially prepared for veterinarians, it may be read with profit by the general practitioner who is not above learning the diseases which are peculiar to animals.

A MANUAL OF PRACTICAL INORGANIC CHEMISTRY. Including Preparations and Qualitative and Quantitative Analysis with the Rudiments of Gas Analysis. Specially adapted to cover preliminary and intermediate university courses and the first three stages of the syllabus of the Board of Education. By A. M. Kellas, B. Sc. (Lond.), Ph. D. (Heidelberg), Lecturer on Chemistry at the Middlesex Hospital Medical School, etc. New York: Oxford University Press. 1910.

The book is designed for those who already have some knowledge of the elementary principles of chemistry. As a laboratory textbook it is adapted to meet the requirements of university courses. The last 100 pages of the book deal with quantitative analyses; and there is an almost complete exposition of the technique including quantitative gas analysis.

INTRODUCTION TO PRACTICAL ORGANIC CHEMISTRY. Including Qualitative and Quantitative Analysis and Preparations. With a Special Appendix on the London University Syllabus, and Schemes of Analysis for Stages 1 and 2 of the Board of Education Syllabus. By A. M. Kellas, B. Sc. (London), Ph. D. (Heidelberg), Lecturer on Chemistry at the Middlesex Hospital Medical School, Formerly Examiner in Chemistry to the Conjoint Board of the Royal Colleges of Physicians and Surgeons. New York: Oxford University Press. 1911.

This volume is a companion to the one on inorganic chemistry with the same general plan and aims. The technique of organic analysis receives detailed consideration, including determinations of molecular weight, volumetric fat estimation, quantitative nitrogen estimation, etc.

VACCINE THERAPY. Its Theory and Practice. By R. W. Allen, M. D., B. S. (Lond.), Late Clinical Pathologist to the Mount Vernon Hospital for Diseases of the Chest; Late Pathologist to the Royal Eye Hospital; Late Gull Student of Pathology, Guy's Hospital. Third Edition. Philadelphia: P. Blakiston's Son & Co. 1910. Price, \$2.00.

This is the third edition of the author's work upon the vaccine treatment. It has been completely re-written and brought up to date. Besides the original work upon specialized immunity, it includes the five years' experience in this line of therapy as is shown by the illustrative cases, taken from the author's clinical material. Careful study of this book should enable the general practitioner, for whom the book is written, to approach with confidence the case requiring active immunization by means of vaccines.

OLD AGE DEFERRED—The Causes of Old Age and Its Postponement by Hygienic and Therapeutic Measures. By Arnold Lorand, M. D., Carlsbad, Austria. Translated, with additions, by the Author, from the Second German Edition. Philadelphia: F. A. Davis Company. 1910.

The work is little more than a theory or set of theories as to the cause of old age, based upon our as yet rather hazy knowledge of the ductless glands. It is written in popular fashion and very evidently intended for the eyes of the layman. We are unable to see that the book has supplied any pressing need.

BOOKS RECEIVED.

VERSES FROM THE SOUTHWEST. By Theodore Clarkson Merrill. Cambridge: Riverside Press. 1910.

THE WORLD OF DREAMS. By Havelock Ellis. Boston and New York: Houghton Mifflin Company. 1911. Price, \$2.00.

THE KING'S EVIL. By Raymond Crawford, M. A., M. D., Oxon., F. R. C. P., Fellow of King's College, London. New York: Oxford University Press. 1911.

IN AND OUT OF PARLIAMENT. Reminiscences of a Varied Life. By Right Hon. Robert Farquharson, P. C., M. D., LL. D. London: Williams & Norgate. 1911. Price, 12s. 6d.

MENTAL EFFICIENCY AND OTHER HINTS TO MEN AND WOMEN. By Arnold Bennett, Author of "How to Live on 24 Hours a Day," "The Old Wives' Tale," etc. New York: George H. Doran Company.

DER GYNAEKOLOGISCHE OPERATIONSKURS AN DER LEICHE. In 16 Vorlesungen. Von Dr. Wilhelm Liepmann, Dozent der Universitaet in Berlin. Mit 387 Abbildungen. Berlin: August Hirschwald. 1911.

DIAET UND KUECHE. Einfuehrung in die angewandte Ernaehrungs-Therapie. Von Dr. Wilhelm Sternberg, Spezialarzt fuer Ernaehrungs-Therapie in Berlin. Wuerzburg: Curt Kabitzsch. 1911. Preis, 5m.

ON WRITING THESES FOR M. B. AND M. D. DEGREES. By H. D. Rolleston, M. D., F. R. C. P., Senior Physician St. George's Hospital; Physician, Victoria Hospital for Children, Chelsea. London: John Bale, Sons & Danielsson, Ltd. 1911.

BAKTERIOLOGISCHES TASCHENBUCH. Die wichtigsten technischen Vorschriften zur bakteriologischen Laboratoriumsarbeit. Von Dr. Rudolph Abel, Geh. Ober-Medizinalrat in Berlin. Fuenfzehnte Auflage. Wuerzburg: Curt Kabitzsch. 1911. Price, 2m.

LA NEURASTHENIE RURALE. Fréquence, Causes sociales et individuelles. Etude psychologique et clinique de la neurasthénie Chez le Paysan contemporain. Par le Dr. Raymond Belbèze. Préface de M. le Professeur Rémond. Paris: Vigot Frères. 1911. Price 3fr. 50.

A TEXTBOOK OF PHYSIOLOGY. For Medical Students and Physicians. By William H. Howell, Ph. D., M. D., Sc. D., LL. D., Professor of Physiology in the Johns Hopkins University, Baltimore. Fourth Edition, thoroughly revised. Philadelphia and London: W. B. Saunders Company. 1911. Price, cloth \$4.00, half morocco \$5.50.

ONE HUNDRED SURGICAL PROBLEMS. The Experiences of Daily Practice Dissected and Explained. By James G. Mumford, M. D., Visiting Surgeon to the Massachusetts General Hospital; Instructor in Surgery, Harvard Medical School; Fellow of the American Surgical Association, etc. Boston: W. M. Leonard. 1911. Price, \$3.00.

THE AMERICAN ILLUSTRATED MEDICAL DICTIONARY. A new and complete dictionary of the terms used in Medicine, Surgery, Dentistry, Pharmacy, Chemistry, Nursing, Veterinary Science, Biology, Medical Biography, etc., with the pronunciation, derivation and definition. Including much collateral information of an encyclopedic character. By W. A. Newman Dorland, A. M., M. D., Member of Committee on Nomenclature and Classification of Diseases of the American Medical Association; Fellow of American Academy of Medicine. Sixth Edition, revised and enlarged. Philadelphia and London: W. B. Saunders Company. 1911. Price, flexible leather \$4.50, indexed \$5.00.